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ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY
ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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180 Dundas Street
Toronto, Ontario
Monday,
July 20, 1981
VOLUME XIX



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ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

ARISING FROM THE USE OF ASBESTOS IN ONTARIO

VOLUME XIX

INDEX OF WITNESSES:

DR. ERNEST DONALD ACHESON	Examination-in-chief	Page 4
	Cross-examination (McNamee)	Page 78
	Cross-examination (Hardy)	Page 81
	Cross-examination (Jolley)	Page 98
	Cross-examination (Bazin)	Page 106
	Examination (Laskin)	Page 120
	Cross-examination (McNamee)	Page 121

INDEX OF EXHIBITS:

EXHIBIT # 28	Dr. Acheson's report on asbestos to the Simpson Committee	Page 4
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THE FURTHER PROCEEDINGS OF THIS INQUIRY
RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Ladies and gentlemen, may I call us to order. Before I greet our witness this morning, learned counsel has some schedule announcements.

MR. LASKIN: Let me just deal with a few matters which affect our future course of hearings.

The most immediate is July 28th, which is a week tomorrow. On looking at Dr. Holstein's material...and you may recall we had two days set aside for him...it became fairly clear that we can finish his testimony in one day.

Mr. Finkelstein, who we all would like to hear from, has been good enough to agree to testify on Tuesday, July 28th, so that we will hear his testimony a week tomorrow.

Now, with respect to August, the dates that I can formally tell you about are the following: August 13th and 14th, which Mr. Hardy's client has requested, and I take it we may start in the afternoon of August 13th, at two o'clock.

Then the QAMA has requested August 19th and 20th. We have Dr. Alison McDonald on August 27th. August 24th has been tentatively set aside for the Toronto Occupational Resources Center,

MR. LASKIN: (cont'd.) although they haven't given me a final commitment on that.

And then there may be one or two other witnesses the Commission wishes to call. Everyone is aware that we have tried to have Dr. Dement come and testify. We have been in touch with him, he is in the midst of changing jobs. He is going to a new job at the end of July, and he wants to speak to his new employer about testifying, but I hope that we will be able to have him here.

Those are my announcements.

Have I got everybody's schedules correctly?

DR. DUPRE: Any other matters?

Well, may I please, on your behalf, greet most warmly Dr. Acheson, who has come a long way on his way to an even further stop. This is a stopover on his way to Japan.

Dr. Acheson, I greet you not only as an expert who has very kindly co-operated with us by agreeing to give sworn testimony, but I greet you as well as a veteran, so to speak, of an unofficial public inquiry into the matter of asbestos.

You are most welcome here indeed, sir.

Miss Kahn, would you swear in the witness, please?

ERNEST DONALD ACHESON, SWORN

EXAMINATION-IN-CHIEF BY MR. LASKIN

Q. Dr. Acheson, let me just put before you, before we start, a brief of some of your publications, and it of course includes at tab five your report on the ill effects of asbestos to the Simpson Committee. That will be exhibit twenty-eight.

EXHIBIT # 28: The abovementioned document was then produced and marked.

MR. LASKIN: Q. I understand you are going to, at least at the outset, briefly make some preliminary comments on some of the matters referred to in that document.

A. Yes, sir. Would you like me to do that now?

Q. Please proceed. Sure.

5 A. I thought it might be helpful if I were to refer briefly to each of the sections in the second volume of the report on asbestos, perhaps make one or two points and make some remarks about whether I feel that any additional scientific information which has been published since the report was completed in 1979 has altered the conclusions that we reached at that time.

10 Perhaps I should say that any remark that I make today is, of course, a personal remark, because I cannot speak on behalf of the Simpson Committee, which has completed its work.

So this is personal testimony, but testimony based, of course, on the experience of the Simpson Inquiry, together with my own judgement on what has happened since.

15 Well, if we look at the first section of the second volume of the report, dealing with classification, distribution and usage of asbestos, I would simply like to make two points. The first is to remind the Commission that asbestos is unique among carcinogens currently...and fibrogens...currently known to man, in that it is the only carcinogen, so far as I know, which resists
20 destruction and is virtually indestructible for practice purposes.

The second points relates to the usage of asbestos, and I would like to point out to you that while chrysotile has a very large number of uses, amosite asbestos has only two, in the United Kingdom at any rate. One is the manufacture of fire resistant boards, insulating boards, and the other is its use in
25 pressure piping.

I think there are very considerable practical implications of this in terms of control. Chrysotile has such a wide range of usage that any control of its usage, if this were thought to be indicated, would be quite difficult.

30 Amosite, on the other hand, one only has to look at two uses, namely, as I've said, the manufacture of fire resistant

THE WITNESS: (contd.) boards and the manufacture of pressure piping from asbestos cement.

5 Crocidolite, of course, is no longer imported into the United Kingdom, so that it has no current usage in the United Kingdom, although of course it is present still in insulation materials, in power stations, in public buildings. It's present in certain public service vehicles such as locomotive...such as railway carriages, and no doubt also in some ships designed twenty or thirty years ago.

10 I pass on to section two, asbestos-related disease. I do not have to remind the Commission that there are three principal and universally-agreed asbestos-related disease - asbestosis, lung cancer and mesothelioma.

15 Asbestosis has been associated particularly with very dusty operations. Lung cancer is unique in that it is related also to smoking, and smoking and asbestos interact synergistically in its etiology. Mesothelioma differs from lung cancer in that there is no tobacco effect, and it is of particular public importance because as it is an extremely rare disease, other than in asbestos...in persons who have been exposed to asbestos...it's relationship to asbestos is much more obvious than is the case in lung cancer.

20 Since the publication of our report, I feel personally that the evidence which we expressed reservations about at the time relating certain alimentary tract cancers to asbestos is, if anything, a little weaker.

25 There have been a number of studies published in which there has been no excess of alimentary tract cancers noted. For example, the studies of the Nottingham gas mask workers, the friction material workers of the Ferrodo factory in England, and also Dr. Dement's study. In none of these has there been a significant excess of alimentary tract cancers.

30 It seems thus that there may well be an additional

THE WITNESS: (cont'd.) factor which we do not understand, which is required to operate to determine the occurrence of attributable alimentary tract cancer in an asbestos worker.

5 We expressed the view in the report that the balance of evidence was in favor of a relationship between asbestos and carcinoma of the larynx. I am not aware of any subsequent publication in this field since the report was published, and I continue to hold the view that certain cases of carcinoma of the larynx may be attributable to inhalation of asbestos dust.

10 Passing on to the relationship of medical effects to fiber type, that's section three, there has been a lot more evidence published on this point. I would like to refer, first of all, to animal data and then to the human evidence, and finally to make one or two comments in which I attempt to reconcile the animal and the human data.

15 First of all, with regard to the animal data, I am of the opinion that there is no doubt that it has been shown conclusively that inhalation of chrysotile, of amosite, and of crocidolite dust all produce malignant tumors in animals, provided the specification of the dimensions of the fibers falls within a certain range.

20 I refer to inhalation experiments.

I then go on to refer to installation experiments, and I consider that the evidence also shows that chrysotile, amosite and crocidolite all produce mesotheliomas when fibers are instilled into the pleural space.

25 It seems that we are concerned particularly with fibers with a diameter range of point one of a micron, to about two point five of a micron, although these are approximate limits, both the lower and the upper limit, and particularly in relation to fibers of twenty microns and longer...between twenty microns and eighty microns in length.

30 I also accept as proven the animal evidence which

THE WITNESS: (cont'd.) shows that less chrysotile remains in the lung after inhalation than amphibole dust.

So there is my own personal review of the animal data.

Now, turning to the human evidence, the view expressed in the Simpson Report that in respect of mesothelioma amphiboles, particularly crocidolite, are more dangerous than chrysotile, has been strengthened by a lot of new data since...published since 1979.

First of all, Dr. Berry's study of the Ferrodo factory, which he has given evidence about to the Commission, showed that in a factory in which chrysotile had been used, except in one area, almost all of the mesotheliomas were associated with the area in which crocidolite had been used.

Dr. Alison McDonald has published a preliminary report in which she compares three industrial cohorts - one is the same factory which Dr. Dement has studied in which only chrysotile was used, a second is another factory manufacturing friction materials from chrysotile, and the third is a factory in which a wide range of asbestos products were produced, including textiles, in which chrysotile, amosite and crocidolite were used.

All are well-established factories, all in the United States, with, one would imagine, similar standards of diagnosis applied to the patients, and with the exception of one case in Dr. Dement's factory, which he also refers to, of mesothelioma, all the mesotheliomas were found...I think it was eighteen of them...in the factory which used crocidolite, amosite and chrysotile.

Dr. McDonald has also published a further study of mesothelioma in the Province of Quebec. There had been a previous study which we referred to in our report. It comes to the same conclusion as the previous one, that in spite of the fact that some millions of tons of chrysotile have been mined in Quebec for many years, for all of this century, almost all the cases of mesothelioma which are industrially related have occurred in the City of Montreal where there has been mixed exposure of the work

THE WITNESS: (cont'd.) force in respect of amphiboles and chrysotile.

5 There has been a further study of...a case control study of mesotheliomas in North American, which...by McDonald and McDonald...which shows a very remarkable increased relative risk in insulators and people working in shipyards, which again suggests exposure to amphiboles.

10 Finally I would refer to Dr. McDonald's case control study of lung tissue of persons with mesothelioma, comparing these with persons with secondary lung tumors, looking at the electron microscopical profile of the fibers present, in which he has demonstrated a relationship between mesothelioma and the presence of crocidolite and amosite, but not of chrysotile.

15 Now, as I mentioned before, it is well known that chrysotile does not remain in the same proportion in the lungs as do the amphiboles. The reason for this is not known, but it is, I think, universally agreed.

20 The fact remains that the cases and controls did not differ in the amount of chrysotile that was present, and it seems to me that it's very difficult to argue that chrysotile can have had a role in the production of these mesotheliomas.

25 Now, on the other side, Julian Peto, who will be giving evidence to the Commission, has published a paper which is in the latest report from Lyon, of the conference that was held I think in 1979 or 1980, on manmade mineral fibers and asbestos, in which he suggests that peritoneal mesothelioma is associated with amphiboles, and if you set aside peritoneal mesothelioma, there is no difference in incidence between pleural mesothelioma in chrysotile workers and pleural mesothelioma in amphibole workers.

30 The study which he uses in his comparison in that paper is the study of Newhouse and Berry from the Barking factory, in which the work force was heavily exposed to all types of asbestos.

In fact, he makes inappropriate comparisons because

THE WITNESS: (cont'd.) he compares selected cohorts from Rochdale, selected on the grounds of long exposure in that factory, and he compares them with a work force in which the majority of the people were exposed for relatively short periods.

The Rochdale data, if you look at it in the round, suggests that there has been an appreciable risk of pleural mesothelioma in that factory. The conclusion that the Simpson Committee reached about the Rochdale evidence was that at this point in time it is impossible to recover the truth about the extent to which crocidolite was used in that factory. It is certain that it was used, and there are differences of opinion expressed by different witnesses as to the extent to which it was used.

My own view is that it will be necessary to examine the lungs of the cases and controls before one can conclude that the thirty...I use the word thirty in rough terms, it's approximately thirty cases...of mesothelioma which have been described from the Rochdale factory are attributed to chrysotile....which is Julian Peto's conclusion.

Now, we are then left with a problem, and that is that, as I said at the beginning of my remarks on this section, we have indubitable evidence from animal experiments that all three fibers...the three fiber types...can produce pleural mesothelioma.

On the other hand, we have, as I see it, indubitable and strengthening evidence that up to the present time in man mesothelioma has been associated particularly with crocidolite, to a lesser extent with amosite, and least with chrysotile.

I would like to make clear that I am not suggesting this is a difference in kind, even in man, but a difference in degree..but a difference which has been, up to the present, at least one order of magnitude, at least a tenfold difference. Dr. Alison McDonald suggests a thirtyfold difference in incidence - I wouldn't

THE WITNESS: (cont'd.) quarrel with that.

Well, now, how do we reconcile these points?

5 My view is that it may very well be that fiber for fiber there is no difference in risk...if you take account of the specifications which are necessary, in fiber size, to produce mesothelioma, as has been demonstrated in animals.

10 But I think that crocidolite has a much greater propensity to create a dangerous dust cloud than chrysotile. That is to say, less energy is required to open crocidolite to the extent that it creates a dust cloud of fibers of the appropriate specification than chrysotile.

15 Now, there is of course a word of caution that has to be made here, and is clearly made in the Simpson Report. That is that we are looking at the record of chrysotile in respect of mesothelioma in relation to its properties, the dust properties, of up to, shall we say, at the most, ten years ago.

20 If there has been a tendency in the industry to use chrysotile in the last ten years in a way which creates finer fibers and a dust cloud which is closer to that which has been characteristic of crocidolite in the past, then we may expect in the future to see chrysotile causing more mesotheliomas per thousand men and women exposed than in the past.

25 I believe there is a key...there is key evidence which your Commission ought to seek in relation to this point. The Simpson Committee sought this from the asbestos industry in Quebec, and were not able to get the information which we had hoped to obtain. We understood that the situation is something like this, but we did not see figures to support it: Namely, that with the exception of asbestos used in asbestos cement, there has been a tendency over the last twenty years for the fiber to be opened to a greater extent and to be milled in a way which would tend, on average, to create dust clouds more closely
30 similar to those of crocidolite.

THE WITNESS: (cont'd.) But on the other hand, in relation to asbestos cement, the trend has been in the opposite direction.

5 DR. UFFEN: Would you mind a small interruption here? You said you were unable to get the data. Did they not exist, or they were unavailable to you?

10 THE WITNESS: It's not possible for me, sir, to distinguish between those two possibilities. I think the data must be available for market reasons, and it may be that it would have been too costly or difficult to put it together in a form for a royal commission in another country. But I think that in Canada it might be easier for you to obtain it.

I suspect that the data must be available, as I say, for commercial reasons.

15 I think that the case in relation to the more dangerous properties of the amphiboles than chrysotile rests principally on human data about mesothelioma.

20 Such data as exists about lung cancer and asbestosis fits in general with the same view, with the possible exception of the Dement study, which I will come back to. But I don't think one can place a great deal of reliance on the data...it's in rather small quantity...about lung cancer and asbestosis, and the relative risks of the different types of fiber.

25 A word about amosite. Amosite, the data is...there is much less data about the relationship of the fiber to mesothelioma risk than for crocidolite. We have a series of papers from the New Jersey insulation materials factory, which was concerned with the manufacture of marine insulating material during and after World War II. We have the paper which I have referred to in which Dr. Alison McDonald and electron microscopists compared the lungs of persons with mesothelioma, in terms of
30 fiber type, with controls. This has been published since the report and it shows that there is a relationship not only with crocidolite, but with amosite.

5 THE WITNESS: (cont'd.) We know that the American insulators with very high relative risks both for mesothelioma and lung cancer, whose experience has been published by Dr. Selikoff and colleagues, have been exposed to amosite and chrysotile and not, to any extent, to crocidolite.

10 I am personally concerned in two studies in the United Kingdom, one involving a factory in Scotland in which amosite only was used in the manufacture of insulation board, and the other in which amosite and chrysotile was used in the manufacture of insulation board in London. We hope to publish a preliminary report about the results, in particular in relation to mesothelioma, within the next few weeks.

15 Turning to the question of dose-response relationships, I think one has to make a decision at the beginning, and we made that decision in the Simpson Committee. Whether we made the right one, I don't know.

20 I think there are two alternatives: One is that you can take the view that bearing in mind the changes in technique in measuring dust, the need in all historical cohort studies, both in North America and in Europe, to transform data from particles to fibers, and all sorts of other practical difficulties about changes from fixed static sampling to personal sampling and so on, one possible view to take is that you should discard all the dust data and not attempt to come to any conclusion about a dose-response relationship, or about the question of a threshold.

25 I know that some colleagues in this field, notably Dr. Selikoff, have taken that view.

30 The other view is to try with as much skepticism and exercise of critical faculty as possible to make some conservative estimates of dose-response relationships from such data as there are, and we decided - rightly or wrongly - to take the second view.

5 THE WITNESS: (cont'd.) When you come to it, there is actually very little data...or there are very little data on which it is possible to make any estimates between dose and response, and they are the Quebec miners studies, Dr. Enterline's studies of process workers who survived to retirement age - a highly selected group, pensioners only, a single point based upon very small numbers, calculated by Julian Peto on the basis of the Rochdale data, and more recently, Dr. Dement's studies.

10 They all relate to lung cancer. There is nothing quantitative on mesothelioma. There is one set of quantitative data...there are two sets of quantitative data about asbestosis. One is Dr. Berry's analysis of the Rochdale data, and the other is Hans Weill's data from New Orleans.

15 For the very good reason that Dr. Selikoff doesn't accept any historical dust levels, there are no dust levels mentioned in any of his reports. That relates to the amosite insulators in New Jersey, the various studies of the insulators... the North American insulating trade unions.

20 Well, to cut a long story short, our view was that, first of all, there was no evidence that in respect of lung cancer there was a threshold below which exposure to asbestos conferred no increment in risk.

I haven't seen any data which would make me change my view on that.

25 Dr. Berry is very careful in his paper about the Ferrodo works to point out that although there appears to be no excess mortality from lung cancer in the group as a whole, there may well be an increment in the group exposed to the highest fiber values when he looks at the material in terms of dose, and he doesn't exclude the possibility that there is a dose-response relationship.

30 So with regard to a threshold and lung cancer, my view has not changed, and the data published subsequently to the

THE WITNESS: (cont'd.) report are all consistent with the linear hypothesis, and Dr. Enterline has accepted the linear hypothesis, although he didn't accept it in his earlier publications.

With regard to asbestosis, again a linear hypothesis fits the data both from Rochdale and from New Orleans. One may be less certain that there is no threshold in relation to asbestosis, in view of the fact that although it is known that a very large proportion of the urban population of industrialized Europe and North America have substantial numbers of asbestos fibers in their lungs, they do not at autopsy have evidence of asbestosis.

So it may very well be that there is a threshold in respect of asbestosis.

With regard to mesothelioma, the data which have been published about the gas mask workers and the West Australian crocidolite miners are all consistent with a dose-response relationship. The only measure which can be used is duration of exposure. There is no measure based on fibers that I know of.

But it is consistent with a dose-response relationship, and it may be linear.

I think there is one point I forgot to mention when I was talking about fiber type, and that is that again, looking at Julian Peto's work, when he makes comparisons between the gas mask workers and the chrysotile miners, he doesn't seem to me to make sufficient allowance for the fact that the crocidolite workers, on average, worked two years or less, both in Canada and in Britain... gas masks were only made for a short period...whereas the miners spent...most of them spent many years in the mines, and yet you have this very substantial difference in the incidence in mesothelioma between them.

To turn to risks outside industry, and again look at each of the principal asbestos-related diseases in turn - mesothelioma, asbestosis and lung cancer - with regard to

THE WITNESS: (cont'd.) mesothelioma, we of course know that nonindustrial exposure has occurred. There are some thirty-five...again, approximately...published cases of domestic mesothelioma. There are more cases of neighborhood mesothelioma. By neighborhood I mean mesothelioma due to exposure to asbestos in the neighborhood of mines and factories in persons who have neither worked in the factory or mine, nor has a member of their immediate family.

It is notable that most of these cases, once again, are related to exposure to amphiboles, either alone or in mixtures. There are literally a handful of cases, less than ten, there might even be less than five, associated with the neighborhood of chrysotile mines or with persons exposed to chrysotile alone, bringing chrysotile home to their families.

Such cases, together with the cases in persons who have only worked for a few months making gas masks, have led to considerable public anxiety. This anxiety may be greater than is necessary, because it may well be that the doses have been quite substantial.

I am not suggesting for a moment that this doesn't suggest that this substance is dangerous. Indeed, it does. It shows that its proclivity to create dust and to create dangerous dust is very great, but one has to bear in mind that in the time we are talking about - twenty, thirty years ago - men and women working in asbestos mills brought their clothes home to homes which didn't have vacuum cleaners such as are used today, and once it was in the home...unless it was washed down the drain or went out of the window, both of which would be pretty inefficient...it stayed there. It's quite likely that in the homes of asbestos workers there were really quite dense aerosols of asbestos. It's unfortunate, I think, that we...that no one has attempted to mockup the situation and see just what these levels were.

THE WITNESS: (cont'd.) There is one record which Dr. Selikoff made from the home of an ex-amosite worker some years after the man had stopped work, and it's referred to as a footnote in the report...it's referred to in nanograms...but the conclusion was that there was a substantial amount of dust in the home of this exworker.

Anytime anyone dusted or...the dust cloud blew up.

However, the fact remains that the number of mesotheliomas...mesothelioma is, in any event, a rare disease if you take the country as a whole...either the United States or Britain..and the proportion of the cases that can be attributed to domiciliary or neighborhood exposure is very small.

So that for various reasons, in our report we did not feel that there was or was likely to be a major public health risk outside industry in relation to mesothelioma. Nor, for the reason I have mentioned, in relation to asbestosis.

For lung cancer, the problem is more difficult because it's a common disease and you can't easily pick up asbestos-attributable cases. However, by extrapolation from the quantitative data, using the linear model, we made various calculations which are set out in table thirty-six of the report, and to cut a long story short, our view is that in general the dangers of asbestos in the open air are minimal, even in our great cities, the danger in buildings, with one or two important provisos, are quite small.

Table thirty-six in volume two of the report, and one or two other tables, deal with the question of asbestos in houses and public buildings.

In relation to data about asbestos aerosols in public buildings, we concluded that it was highly desirable that there should be more data. We had to deal with one survey in England, which was not totally satisfactory. So far as it went, it gave cause for reassurance.

However, there is no doubt that buildings in which

THE WITNESS: (cont'd.) amphiboles have been used, in particular as a sprayed insulating material, there have been dust levels reported above the industrial standard.

5 In certain naval stores in England, in which crocidolite had been used as the insulating material on the walls, levels above the industrial standard have been found. Certain types of insulating material, including sprayed insulation and insulation boards, are liable to damage. We had evidence about this from the Institute of Surveyors in England, and they can give cause for concern.

I would like to make a particular point, and it's dealt with on page forty-six, about risks to children. I know this is of interest in Ontario. We were asked to make a specific study of it.

15 There are two points I would like to make. A small number of the published neighborhood cases of mesothelioma, and ten of the domestic cases of mesothelioma in the literature, which have arisen in adult life, have been ascribed to exposure in childhood at home or in the vicinity of works and dumps. I would like to make an exception to my general statement about the fact that there is generally not a problem about asbestos in the open air, and that is in the neighborhood of dumps of asbestos waste where very high counts have been found in certain circumstances in the United States and the United Kingdom, and every measure must be taken to make sure that asbestos waste is not exposed to ambient wind currents, and to children playing on these dumps.

20 The second point I want to make is that we made a special study of cases of mesothelioma reported in children. There are approximately ten cases reported in the literature.

25 Previously it has been the accepted view that these are nothing to do with mesothelioma in adults. Partly, I think, because of certain idee fixe about latent period.

30 In fact, the occupations of the fathers of nine

THE WITNESS: (cont'd.) of the children were as follows: Molder, plumber, electrician, mechanical engineer, farmer, building constructor, lumber dealer, salesman and ceramics engineer in a factory utilizing chrysotile and amosite.

Of these nine cases it is possible to assume from the nature of the employment, parental exposure to asbestos is certain in one and probable in five. My own view is that it is likely that the majority of cases of mesothelioma in children... and let me emphasize once again that we are now dealing with an extremely rare disease of which less than twenty cases have ever been reported in the literature...nevertheless, I suspect that a proportion of these are attributable to asbestos exposure from domiciliary exposure.

Finally, public policy. I think I'm going to, if I may, make one or two general conclusions in a moment, but at this point I would just like to make one or two practical points.

I think one is faced with the possibility of suggesting a single level for the whole of industry in relation to a particular fiber type or all fiber types, or saying there are demonstrably different risks in relation to the asbestos-cement industry, the textile industry, the friction materials industry, therefore should one approach these with different control limits.

We considered this in the Simpson Committee, and decided against it on the basis that it would have been extremely complicated in terms of legal definitions, and on the general principle that simple law is more likely to be enforceable than complicated law.

The second point I would like to make is that in setting any standard account has to be taken, in my opinion, of the fact that there has been a de facto tightening of the standard in the last ten years. The factor involved is arguable, but it is somewhere between a factor of two and a factor of eight, or a factor of two and a factor of five.

5 THE WITNESS: (cont'd.) So what I'm saying is, although in Britain the two fiber limit was set in 1968, it is now being enforced as a point four fiber limit in terms of the standards of measurement of 1968. There has been perhaps a fivefold, it varies in different circumstances, but on average something like a fivefold tightening de facto without any de jure change in the standard.

10 It took us quite a long time to get this one clear in our deliberations, but the expert evidence of Dr. Steel, which you will see in volume two, as a highly-respected, totally-independent hygienist, was quite clear on this point.

15 Thirdly, if you set a standard...and let's take the present standard so as not to be at all contentious...the present standard in England of two fibers. This does not mean that every man...in fact it means that many fewer than all the people in the factory at which that standard is enforced will be exposed to two fibers. Perhaps a tenth of them.

20 This is very important, because if you come to the sort of conclusion that we did at the end of our study, we gave our main committee a series of options at different levels. If you take the radiation industry, for example, they have set a standard of five rems per year, which they believe if a hundred men are exposed to five rems per year, instead of twenty percent of them dying of cancer, twenty-two percent of them will die of cancer. That's the compromise that the international radiation industry has come to.

25 But, only a tenth of the men will actually be exposed to five rems for fifty years, so if you take the industry as a whole the estimate is that enforcing a limit of five rems will increase the mortality not by two percent for cancer, but point two percent.

30 So there is this additional dimension that has to be taken into account when one is debating what standard to reach.

THE WITNESS: (cont'd.) I will conclude my remarks there, sir, if I may, for the time being.

MR. LASKIN: Dr. Acheson, thank you very much.

I wonder if we perhaps ought to give the witness about five minutes, since he has been talking for some time.

We'll give you a little bit of a break, Dr. Acheson.

DR. DUPRE: Shall we rise until 11:25?

MR. LASKIN: Sure.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

MR. LASKIN: Q. Dr. Acheson, before we get back to the health effects issues you addressed, can I just understand your own role in relation to the Simpson Committee? As I understand it, you had a dual role, in a sense. You were first of all a member of the advisory committee itself?

THE WITNESS: A. That's correct.

Q. That committee had representatives of industry, labour and a number of medical and other experts?

A. Yes, that is so. It also had a representative of the public, of the consumer interest.

Q. And was chaired by the person who is himself chairman of the Health and Safety Commission in England?

A. That is correct.

Q. Is it the Health and Safety Commission that is ultimately responsible in England for promulgating standards, for example with respect to asbestos?

A. Yes.

Q. You were also a member of the medical working group?

A. Yes.

Q. It was in that capacity that you prepared the

Q. (cont'd.) report which is in volume two?

A. That is correct.

Q. I take it from looking at your mandate in
5 volume two that questions as to whether the industry could meet
any proposed standard, questions as to the effectiveness of
substitutes and so on, were not matters within your mandate in
writing the report, at least?

A. That is correct.

Q. Was it the committee itself that looked at
10 those issues?

A. Yes, the way it worked was as follows: The
medical subcommittee asked me to write a paper reviewing the
health effects, which I did with the help of Dr. Gardner, who is
a statistician. There was then the question of whether the medical
15 committee as a whole would accept the report or not, and they did.
They then presented, the medical working party presented the
report as being pretty close to the corporate view of the group
as a whole, to the main committee, and at this point we in the
medical working party and in the paper which I have written, have
presented the committee as a whole with a series of options, basically,
20 from which they were able to choose. The choice depended really
upon the result of the debate in which labour on the one hand,
industry on the other, and the public interest and the other
experts - hygienists and so on - each had their view.

I note subsequently...and I should say also that
both in the medical subgroup and to a lesser extent in the main
25 group, the views which I had expressed in this report were
subject to cross-examination by other experts.

Q. On the committee?

A. On the committee.

Q So your report and you were then tested
30 yourself by being cross-examined?

A. Yes, indeed. Indeed. Not cross-examined in

5 A. (cont'd.) the formal sense such as occurs in a royal commission, but we were subject to...all the views were tested carefully by other colleagues, medically qualified colleagues or hygienists or civil servants who had a particular expertise in enforcement and so on.

10 When it came to the point that views were expressed as to what reduction in the standard industry could or could not cope with without having to close down its operations, I was very interested to find that there was no expert cross-examination and there was no independent evidence taken. Clearly there was a different view expressed by the labour unions to industry, but it wasn't on the basis of what I would describe as independent expert advice, at least not at the committee. There may have been advice behind the scenes.

15 It seems to me that in our developing process...and the Simpson Committee was a development on what had happened previously, and is now quoted as an example of progress...my own view is that there is a need for further progress and I hope that next time in the United Kingdom this sort of group is brought together to advise on an issue of this sort, that there will be an independent economist who will be able to subject the statements of industry about the effect of tighter standards on their costs to cross-examination and expert scrutiny.

20 And the same applies to substitutes. When it came to the question of whether it was possible to consider that a particular sort of asbestos might no longer be used in a particular process and something else used instead, there didn't seem to be an independent source of testimony available to the committee.

25 I should say that the Commission didn't notice that there was anything missing. It's something subsequent and it's a purely personal view that I am expressing, that it seems to me that that is as important as is the medical evidence, and should be subject to the same sort of objective scrutiny.

DR. UFFEN: Could I follow this a bit?

MR. LASKIN: Sure.

5 DR. UFFEN: At the committee meetings where there were representatives of labour and the public and so on, did those people have questions? Would they have been in a position to put questions?

10 THE WITNESS: They certainly...it would have been, I think, totally in order for them to do so, but I don't think any of us had the feeling that the actual evidence on which statements of cost were based by industry could be scrutinized, or were available for scrutiny.

I mean, industry said if you reduce the standard from X to Y, we will be able to cope, but if you reduce it to Z, you will close down our operations on the grounds of cost.

15 Now the evidence on which that statement was made was never seen by the committee.

DR. UFFEN: One of the remarkable things, the notable things, about the British procedures that impressed me was the degree of understanding where unanimity would eventually emerge on these committees.

20 I interviewed one or two people on these, and I was quite surprised to find that after an initial breakin period where they got to know one another, they did reach consensus and so whatever the report was, it had the approval of the other representatives.

25 THE WITNESS: That is correct. The proposals made in the report were the result of a consensus. No one withdrew or required to put in a minority report or to make a reservation on a major point.

MR. LASKIN: Q. May I ask you what the present status of the recommendations of the Simpson Committee are?

30 THE WITNESS: A. The present status, as I understand it, is that they are still undergoing the process of

THE WITNESS: A. (cont'd.) public consultation, and that progress has been slowed down because of the need to relate the recommendations to those of the EEC as a whole.

5 I am not competent to explain the details of this, but I do understand that there are differences of opinion between Britain and other members of the EEC about these recommendations, and this is slowing down the process.

10 Q. Is there within Britain an ongoing mechanism to continually review this kind of issue, or was the Simpson Committee when it was struck something out of the ordinary, or was it part of the ongoing machinery of standard setting?

A. It was a special committee set up to respond to public anxiety about a particular problem which had emerged in the public press in relation to a particular factory.

15 The Health and Safety Commission does have a number of standing committees, including one on toxic substances, which includes representatives from industry, labour and experts, and as I understand it, though I am not a member of it, takes as they appear problems...for example, recently it has been discussing the problem of formaldehyde. So that there are standing
20 committees which advise the Health and Safety Commission.

Q. On an ongoing basis?

A. On an ongoing basis.

25 Q. Can we turn to some of the health effects issues, and before I forget about it can I just ask you one or two questions about your comments concerning children, which is of interest to this Commission.

I suppose I have two questions. I note at paragraph 239 of your report, where you discuss this in some detail, you raise the speculation that the tissues in children...and I take it lung tissues...may be more susceptible to carcinogens than those of adults.

30 I am wondering whether that matter was pursued by

Q. (cont'd.) your medical committee and whether you can help us or elaborate on the speculation that you raise in the document?

5 A. I am afraid I haven't got any additional information to give the Commission on that point. At the time, I consulted experts and was advised that views on this point were not unanimous, but that at least it would be prudent to take into account the possibility that children might be more susceptible than adults from the immunological point of view.

10 Q. I take it from that there are some people who have looked at the question who believe there is a greater susceptibility, and others who do not?

A. I think that is so.

Q. The second question I wanted to ask you is...

15 DR. DUPRE: Just before we go there, counsel, the question of the susceptibility of the young is indeed raised in paragraph 239 on page forty-six. It also comes up as a footnote on page forty-four, where there what is addressed is a possible susceptibility not only in the very young, but of the aged.

20 Can I take it from your answers to Mr. Laskin that such inquiries as you made of experts on this point were not so much asbestos-specific as they were related to perhaps a general susceptibility of both the young and the aged to carcinogenic substances of any kind?

25 THE WITNESS: That is correct, Chairman. There is no evidence to suggest...there is no evidence to guide us about the relative susceptibility of children and adults to asbestos. That is certain. And the evidence that I have obtained from experts related to much wider considerations about susceptibility to cancer in general...and indeed to infections.

30 The only evidence that I know of which relates asbestos-related disease to exposure in childhood is summarized

THE WITNESS: (cont'd.) in paragraph 239.

MR. LASKIN: Q. You refer to the rare mesothelioma and you commented on it before, and during your comment I took one of the points you made is that perhaps in the past people hadn't recognized that these childhood mesotheliomas may be asbestos associated because they were hung up with the latency period?

THE WITNESS: A. Yes.

Q. Are you taking that comment any further by suggesting that perhaps in children the ordinary kind of latency period that we might see with respect to mesothelioma may not always be present?

A. I think that I have been persuaded by the statisticians...and I know that both Julian and Richard Peto would agree with me on this point, because they hold this view... that there is no...that it is incorrect to think of a fixed latent period in respect of cancer, that one should think of the probability of a person developing a cancer in relation to a factor, and that this probability may be greatest at a certain interval but it is not excluded by any means that a case of cancer may occur after a very much shorter period than is generally the case.

I hope that's clear. So that I think that one should never exclude the possibility of cancers occurring after much shorter periods than is generally accepted as the latent period.

Q. Dr. Nicholson, when he was here, put forward a theory...perhaps I've misunderstood him...in roughly similar terms, but as I understood his evidence, really restricted it to the question of lung cancer. Do I take it that you are suggesting that the same kind of argument may well apply in respect of mesothelioma?

A. I must say that I am an empiricist rather than a theorist. I think the theory usually follows the practice in

5 A. (cont'd.) this field. I was impressed, and I'm
the only person to have been impressed, I think, in the
occupations of the fathers of this group of children. That is
really my point, and I have to say that it's not generally
accepted that this is relevant. My colleagues regard this as
coincidental, and point out that there were no controls, which
is true. It is very difficult to think how one could get
controls when one is reviewing a series of papers, each of which
10 report one or two cases over a period of the last twenty years,
mostly in the United States. But these do not seem to me to be
representative of the spread of occupations in North America.

15 They seem to me to be biased towards asbestos-
related occupations, and one has to bear in mind that it is
unlikely to be conscious bias on the case of the authors, because
all of them are of the opinion that mesothelioma in children
has nothing to do with asbestos.

DR. DUPRE: Counsel, if I could just step in.

MR. LASKIN: Sure.

20 DR. DUPRE: I have one more question on the
susceptibility of the young and of the aged to carcinogens
generally, or for that matter to infections.

25 I note that the footnote on the bottom of page
forty-four refers to the very young. Now, when you are getting
some general expert opinion in this broad area, does that
sentence lead me to conclude that there was something like
individuals below sixteen, which would be considered the very young?

THE WITNESS: I think the very young in that
context, one was thinking of the first decade of life.

MR. LASKIN: Dr. Uffen?

30 DR. UFFEN: Could I just tidy up one thing here too?
In the report it says, "There is no evidence up to the present
time of cases of mesothelioma having occurred as

DR. UFFEN: (cont'd.) "a result of exposure in schools, although unsatisfactory conditions have been reported in one school in the U.K."

5 Is that still true? This was written probably three years ago.

THE WITNESS: I am not aware of any, but I have to say to the Commission in all fairness that I have not done a further comprehensive review of the literature since 1979.

10 DR. UFFEN: Could I ask a perhaps naive question, would it be a useful thing to do to initiate a study of people who have been through the school system who are now in their thirties, twenties, in schools at this time, and if this were useful, how many such cases would you have to track down to be of any significance?

15 THE WITNESS: I do not think that that would be a useful study because on the basis of the experience of the large number of children that must have been exposed to the large number of men and women, particularly men, fathers, who came back over the last thirty years with asbestos on their clothes everyday, we know that we are talking about a very rare event indeed. And I feel it's important that the Commission
20 should, with respect, not get this out of proportion....both in respect of the fact that only ten of the domestic cases...we have, I think, less than forty domestic cases of mesothelioma reported in the literature at all, and ten of those...in ten of those was there exposure in childhood. So I would suspect exposure
25 in schools would be very much smaller than exposure in the homes of an asbestos worker, and that in general the risk would be very slight indeed. I am speculating, and I do feel, as I said before, that it is essential that systematic surveys should be made to see what the asbestos levels are in public buildings, including schools, where asbestos has been used as an insulating
30 material.

MR. LASKIN: Dr. Mustard?

5 DR. MUSTARD: Can I just ask a question about this? How good is the reporting data about mesothelioma in the U.K., Canada and the U.S.A., so that I can have confidence that all cases that occur are actually being compiled?

10 THE WITNESS: Mesothelioma reporting is not perfect, and I wouldn't like to make an estimate of the proportion that are misdiagnosed as lung cancer in adults, although it has improved and pathologists tell me that this is now a recognized tumor that everyone has seen in training and is aware of the possibility of diagnosing. One must take into account, however, that in Britain, fifty percent of people die in hospital and perhaps ten percent of all deaths are subject to autopsy. I don't know, some quite small proportion. I should think that's probably true in the United States as well.

15 A malignant tumor in a child, however, I would have thought would be subject to very close inquiry, and I would have thought it would be very unlikely that there are any substantial number of malignant tumors or pleurae in children that are not diagnosed as mesothelioma.

20 DR. MUSTARD: But in the young adult that might not be the case?

25 THE WITNESS: Again, I think malignant tumors in people under the age of fifty are pretty accurately looked into in both Britain, and I would guess North America. I think the diagnosis of tumors in the elderly, for obvious reasons, is less accurate.

But I would have thought that young adults, there is such a big stake in the health and illness of such a person that no mesotheliomas are missed, I would guess, today.

30 MR. LASKIN: Q. Can we turn now to the question of fiber type and the treatment that was accorded that subject in your report and in the Simpson Report itself? I believe I

5 Q. (cont'd.) understand the manner in which you sought to reconcile the epidemiological evidence on mesothelioma and fiber type with the apparent animal evidence which seemed to be contradictory.

10 Can I ask you this question? If in an ideal workplace, or in a theoretical workplace, you were able to control exposure to chrysotile at the same fiber measurement as exposure to crocidolite, and you were able to have the same aerosol in chrysotile as of crocidolite, would, in your judgement, you expect to see any difference in mesothelioma rate?

15 THE WITNESS: A. If the...I'm trying to come to terms with exactly what you are asking. I think what you are asking me is, in other words, if a man or woman inhales a million chrysotile fibers, is he or she likely to have the same risk of mesothelioma as if he or she inhales a million crocidolite fibers. Is that right?

20 Q. Essentially. As I took one of the points you made, it was that amphiboles tend to create a more dangerous dust cloud. They are dustier.

25 If you could exert a greater measure of control on the dust cloud, is that going to affect the relative rate of mesothelioma, or is there something particular about the fiber itself?

30 A. I think the animal evidence suggests that there is nothing of chemical importance between the fibers, but they are of quite different shape and their characteristics are quite different. As you will have seen, I'm sure everyone here has seen, the picture that..I don't know whether I can find it...but there is a nice electron microscope picture here somewhere.

35 Yes, it's on page seventy-seven, and you can see on page seventy-seven in the top lefthand corner, what crocidolite looks like, and amosite on the right and chrysotile...chrysotile divides in a totally different way. It hasn't got a standard

5 A. (cont'd.) minimum spicule. It divides..
can divide, as I understand it...to an unlimited degree. I
have a feeling, and I cannot say that it has been proved, but it
seems to me that it is likely that the amount of energy
required to produce fibers of a dangerous configuration, in respect
of crocidolite, is less than the amount of energy required
to produce a similar fiber from chrysotile.

10 Chrysotile gets involved in tangles, gets
involved in a variation in shape which you don't see with the
amphiboles.

Q. And when you say a dangerous configuration,
I take it you mean some ratios between the diameter of the fiber
and the length of the fiber?

15 A. Yes, and I have mentioned what I think is a
common interpretation of the animal experiments, that we are
talking about ranges of diameter of approximately from point one
micron to two point five microns, and in length greater than
five microns and less than eighty microns.

20 Q. So that do I take it that what it really comes
down to is a matter of fiber dimension and crocidolite is much
more likely than chrysotile to have the dangerous fiber
dimension?

A. Exactly.

25 Q. If you could reproduce in chrysotile the same
kind of dangerous fiber dimension, you may well have the same
results?

A. I agree with that. I believe that is likely
to be the case.

30 Q. Can I just for a moment, pursuing this, take
you to page sixty-seven of your report, and you there list some of
the results of some of the animal experimentation. Can I take
you in particular to table nine, which is the work of Wagner,
and I suppose what I would like to ask you is, because you refer

5 Q. (cont'd.) there to two kinds of Canadian chrysotile, one which you call...or he does...the superfine Canadian chrysotile which seems to produce a greater percentage of mesothelioma than any other kind of fiber.

Can you help us as to what is that fiber? Do you see that fiber in the mines?

10 A. I understand that that fiber was produced for scientific purposes from ore mined in Quebec, but that it is... its specifications are such that up to the present time it has not been used to any extent, commercially. I believe that it would be possible for you to get expert testimony on this question, both from the scientists who prepared the sample at the MRC Pneumoconiosis Unit in Penarth, and also from the Quebec industry.

15 But it relates to a point that I made earlier this morning that I think the precise...the trends in the preparation of chrysotile fibers for commercial use are crucial to the work of your Commission.

20 Q. Apart from your own inquiries into the Quebec situation on that issue, did you make inquiries of any other parts of the asbestos industry to see if you could clarify or obtain any more answers to that question?

A. No. We had industrial members on the Commission, and we were advised at that time that all chrysotile used in the United Kingdom came from Canada, and none from Rhodesia.

25 Q. The other chrysotile, Canadian chrysotile, the UICC chrysotile in table nine, is that, to your knowledge, commercially used?

A. All these preparations were made specifically for scientific purposes, with the idea that they would be used as a standard against which other exposures could be measured.

30 I think the difficulty is that in fact they do not relate in practical terms to anything that is used in commerce, so that in fact they can reasonably be regarded as misleading by some, I think.

Q. Is there, to your knowledge, any animal experimentation that actually takes the kind of fiber that you do find in the workplace and subjects it to animals?

5 A. Yes, there is a study in which different specimens were ground up by hammer milling, and this is referred to in the section on fiber type. I would have to ask for a little notice to find the specific paragraph, but there should be no difficulty in finding it at the next intermission.

10 Q. Sure. Do you recall now offhand what the results were?

A. The results of that particular experiment suggested that crocidolite was more dangerous than chrysotile when it was subjected to hammer milling.

15 Q. One of the matters that come before us in relation to crocidolite is the apparent difference in producing mesotheliomas between the experience at the Cape Mine in South Africa, and the experience at the mine at Transvaal. Is that a real difference, and if so does it fit in with the theory and the argument in the Simpson Report?

20 A. The only source of crocidolite which has been mined continuously over the last fifty years has been the one in the northern part of the Cape Province, which is the origin of the famous, quotes, 'Cape blue asbestos'.

25 The Transvaal crocidolite mines were used only to any extent during the Second World War, when crocidolite was required in vast quantities for marine insulation. I don't place a great deal of reliance on the failure to find many cases of mesothelioma in relation to the Transvaal crocidolite mines. I suspect it's a local matter, because they have been found in Western Australia, which is the other place where crocidolite has been mined.

30 My colleague, Dr. Hobbs, has followed up the West Australian miners and found at least thirty cases of mesothelioma.

5 A. (cont'd.) I think a point that is of greater practical interest is the significance of the comparative lack of mesotheliomas found in relation to the only amosite mine, which is in the Transvaal at a place called Penge, and which I have visited.

10 There may be two reasons for this. One is that amosite is less likely to produce the appropriate aerosol than crocidolite...and I think that is true. Secondly, that the followup of the Africans is not comparable to the situation in the North Cape.

15 I think one has to bear in mind that in both cases there has never been a proper industrial cohort study where a roll of men have been identified and followed. It's very difficult to know what happens. Many of these people are intinerate workers. They may leave the area after a comparatively small period in the mines, and pulmonary tuberculosis is still extremely common and the symptoms of pulmonary tuberculosis and mesothelioma are very similar. So that I don't think that one can conclude that the fact of only five cases of mesothelioma having been reported in and around Penge necessarily represents the truth.

20 Q. Just pursuing that with respect to amosite for a moment, and its relationship to mesothelioma, I take it from reading your report that the principal evidence for coming to the conclusion that it occupied an intermediate position between crocidolite and chrysotile was the experience of Dr. Selikoff's amosite insulators. Is that fair?

25 A. That is correct. Also the experience...the experience of Dr. Selikoff's study of the amosite insulation manufacturers....the factory in New Jersey where marine insulation material was made of amosite during the Second World War and immediately after it. There's that experience, and then one has to separate from that the experience of the insulation workers,

5 A. (cont'd.) the pipe fitters, from the North American labour union involved in pipe fitting who worked with a mixture of amosite and chrysotile. Both these groups of workers got more mesotheliomas than one would have expected if they had had the risk of the Quebec miners, for example.

Furthermore, almost as many got peritoneal mesotheliomas as pleural mesotheliomas, so there seems no doubt at all that amosite is unlike chrysotile in that it causes peritoneal mesothelioma.

10 I also believe that it is more likely to cause pleural mesothelioma than is chrysotile.

Q. Just pursuing...well, just let me pursue those two questions and the Chairman may have some more.

A. Yes.

15 Q. What's the significance, in your judgement, of the fact that amosite seems to produce peritoneal mesothelioma? Is that telling us something about amosite as a fiber?

20 A. I think it may be. The epidemiology...the causation of peritoneal mesothelioma is even more mysterious than that of pleural mesothelioma, but it is even more closely related to amphiboles than pleural mesothelioma.

25 There is no doubt that exposure to chrysotile only can cause pleural mesothelioma. As you know, Dr. Corbett McDonald, in his studies of the Quebec miners, exposed only to chrysotile, has demonstrated, I think, about ten. I forget now whether it's eleven, ten or eight, but shall we say about ten pleural mesotheliomas...which I am perfectly happy to accept are associated with exposure to chrysotile.

30 But no cases, so far as I know, of peritoneal mesothelioma. If you look at a hundred...whatever it is...a hundred, or a very small number of gas mask workers working in Ottawa during the war, there were approximately equal numbers. Again, I'm speaking in approximate terms - peritoneal and pleural mesotheliomas.

5 A. (cont'd.) Peritoneal mesothelioma seems to be especially associated with exposure to amphiboles, but it is not universally so. There is a degree of irregularity about its appearance which is mysterious. For example, no peritoneal mesothelioma has been reported from the Western Australian crocidolite miners; but approximately equal numbers, as I said, in the Ottawa gas mask workers; some seven, I think it is, out of thirty-five from the Nottingham gas mask workers; a lot of peritoneal mesotheliomas from the Barking study where people were
10 exposed to all three; and so far no peritoneal mesotheliomas from the Cape miners.

Now, it may be that there are major problems about diagnosis here.

15 Q. I was going to ask you that. Is there a problem, in your judgement...

A. There may be.

Q. ...with what, diagnosing cancer of the pancreas?

20 A. Cancer of the pancreas...any carcinomatosis involving multiple deposits in the peritoneum can be a difficulty. But I think the study of mesothelioma in Quebec province is a very attractive one from that point of view, because you have basically the same pathologists and the same conditions dealing with the chrysotile miners and the gas mask workers, and you have a totally different spectrum of disease in the two.

25 Q. Why, in your judgement, is amosite more likely to cause pleural mesothelioma than chrysotile?

30 A. My opinion is that it is because the configuration of the fiber and the properties of the aerosol in industrial use up to the present time have been more closely comparable to crocidolite than chrysotile, and if you look at the electron microscopic picture again you will see that amosite is a straight fiber, which again divides longitudinally in a way that chrysotile does not...although the mean diameter, I understand, tends to be greater, as is shown on page seventy-seven, for crocidolite.

5 A. (cont'd.) Some physicists place a great deal of weight on the shape of the fibers in relation to crocidolite and amosite, and in fact they are straight. It certainly seems to me a reasonable thing to point out that they are totally different in shape to chrysotile.

10 If I could just return to amosite and peritoneal mesothelioma, the other group, of course, who have been exposed to amosite in large quantities is the North American pipe fitters, and as you will see in table eleven, they produced a very considerable yield of peritoneal mesotheliomas. I think it's table eleven. Yes.

15 Table eleven, page sixty-eight. You have to turn your book sideways. If you look under mesothelioma, there are two rows. One is pleural, and the other is peritoneal.

20 Now, number seven, column seven, is Selikoff, United States and Canadian pipe fitters, and you can see there were twenty-nine pleural and sixty-three peritoneal mesotheliomas among a hundred and one, and it is generally agreed that these men were not appreciably exposed to crocidolite.

25 Note also column four, which is crocidolite, amosite and chrysotile, and column five, which is Dr. Selikoff's insulation manufacturers in the New Jersey factory...eleven cases, of which six were peritoneal.

30 DR. UFFEN: Could I pursue a point here before we get onto something else?

MR. LASKIN: Sure.

35 DR. UFFEN: You have made quite a careful point about the preparation of the material, as though you were trying to bring to our attention that this is a problem that needs some further discussion. You also mentioned physicists.

40 It would seem to me that someone would have had the idea by now to take some chrysotile, or the others, and take them through different kinds of preparation and examine them under the microscope and see what does happen to the fiber. Has this been done?

5 THE WITNESS: Yes, indeed. There are a number of hygienists who have worked almost exclusively in this...and physicists...who have worked almost exclusively in this field. The person who provided the Simpson Committee, medical subcommittee with data was Dr. Vincent Timbrell from Penarth.

10 I don't know whether he is still..whether he has retired or not, but he was concerned, as I understand it, in the actual specification of the UICC standards, and the SFA chrysotile standard, and also there are persons working in this field in Quebec who I would have thought would advise the Commission where information could be obtained.

15 MR. LASKIN: Q. Can I just ask you one or two more questions about amosite, and in particular Dr. Selikoff's various cohorts? There has been evidence before us that although he had no quantitative measurements that in fact his various cohorts were exposed to very intense doses of asbestos, and sometimes even for short periods of time, and often intermittently.

I suppose my question to you is, is that a possible confounding factor? Does that carry with it any possible explanation for the kinds of results he was seeing?

20 THE WITNESS: A. Yes. Insulators generally, people working at pipe fitting in central heating and marine insulation, do have these very high levels and the point is well taken, could it simply be that they were...that at extremely high levels, whatever the asbestos is, you get peritoneal mesothelioma.

25 I think my answer to that would be that the examination of the lungs of these people by Dr. Alison McDonald shows a difference between the cases and the controls in respect of amosite, and not chrysotile. I find, in logic, it difficult to understand how to interpret that other than that there is an association between amosite and crocidolite and mesothelioma, 30 but to a much lesser extent, if at all, chrysotile.

Q. Let me turn to a slightly different line, but still on the question of fiber type.

As I read your report, and also the Simpson Report, I took it that...please correct me if I'm wrong...but I took it that the main evidence that the Committee relied upon to treat the fiber types differently was their particular relationship to mesothelioma. Is that fair?

A. Yes, I think the evidence in relation to mesothelioma and fiber type is much stronger than in relation to lung cancer and asbestosis. But such evidence as I was able to find at the time the report was written, in which it was possible to look at asbestosis and lung cancer in relation to fiber type, showed a similar trend. I think that has probably stood up, with the possible exception of Dr. Dement's study, which perhaps you would like me to comment on and which I will in a moment...but again, if you look at the gas mask makers, and take into account the fact that they only worked for between two to five years, the relative risk for lung cancer in the Nottingham gas mask workers is very high.

Q. I'll come to Dr. Dement in a moment.

A. Yes.

Q. But as I understood it, when you first of all... well, let's deal with amosite first of all. I take it, at least in respect of lung cancer and amosite, that there are no quantitative assessments that one can look at? Strictly speaking.

A. In respect of anything based on dose, that is the case. I think the Committee was concerned about the very remarkable relative risks from the insulation manufacturing plant in New Jersey, in relation to lung cancer, in terms of period of work, where it was possible to demonstrate an excess mortality for lung cancer in men who had only worked for a month.

That's the nearest that we came to any sort of quantitative data with regard to amosite and lung cancer, and

A. (cont'd.) that is referred to.

DR. DUPRE: Counsel, may I just interject with respect to the gas mask workers?

MR. LASKIN: Sure.

DR. DUPRE: Earlier this morning in your opening remarks, you did comment on the extent to which public anxiety, certainly in this jurisdiction, was to no small degree related to what had happened to the gas mask workers. But as a general proposition, you reminded us, of course, that the kind of public anxiety that came about could be put in context, in the historical context, that exposures, dust exposures, may have been very great. And yet the one thing that I notice on page forty-five, in paragraph 236, where in the upper part of the righthand column you refer to the British and Canadian gas mask workers, what you point out is that not only was the total period of exposure limited, but the nature of the activity involved, as your report says, fitting filter material into containers, suggests that relatively low concentrations of dust, by industrial standards, might have been present in the air.

THE WITNESS: Yes. I'm not sure that the term... in retrospect...that the term 'relatively low, relatively high' is helpful, that it was a helpful sentence to have drafted without saying what one...what it was supposed to be relative to, frankly. I think I would criticize myself for drafting that sentence now.

I find it disappointing that we have not succeeded in convincing anyone of the necessity of making up, to make filling some gas masks from a process which is...the specification is known in detail, exactly what those women were doing in Ottawa, exactly what those women were doing in Nottingham, we know where the crocidolite came from, and I believe that it would be possible for a hygienist to determine what the levels were, without too much difficulty.

DR. UFFEN: Has anyone interviewed these workers

DR. UFFEN: (cont'd.) to ask them just what did they do? Did your committee interview any of them?

THE WITNESS: No.

5 DR. UFFEN: It makes you wonder whether it wasn't just a matter of...maybe they tested every one to see whether they were working. There are all kinds of things, and...

10 THE WITNESS: They were sitting at tables doing a series of line production operations in an assembly line, and what is certain is that the lungs of those who died are shown to be very heavily...have very heavy deposits of crocidolite in them.

15 My own view is that it shows what a dangerous material that stuff is, that doing a comparatively...what should I say...an operation which doesn't involve a lot of energy, a woman sitting and putting something into a container, can create a dust cloud that can be so lethal. My own view is that it would be unlikely...that a similar operation involving chrysotile would be unlikely to be as dangerous.

20 DR. UFFEN: I'm reminded of a period when I used to have to take young recruits into a gas chamber. I wore the damn mask more than probably...there's a half a dozen of us wore these masks before they brought in the other one, and it makes you wonder whether there weren't practices which we would only find out about by talking to some of the workers themselves.

25 This is not a useful analogy, but licking your finger everytime...that's not inhaling it...but something analogous to that.

30 THE WITNESS: Yes. Well, I think the most...if one wished to make an estimate of what the levels were, the best way would be mock up the process, because I think that it would be difficult to get objective testimony at this stage, bearing in mind the period that has elapsed of forty years, and also the climate of anxiety which has subsequently arisen, and the anxiety

THE WITNESS: (cont'd.) which these women will have about their own future health.

MR. LASKIN: Can I come back to your paragraph sixteen, which is just a conclusion, which is at page eleven, and just pursue one of the matters we had started to pursue?

You say, "Evidence in man about the relationship of fiber type to lung cancer is inconclusive, but where it exists indicates that exposure to the amphiboles or to mixtures with chrysotile rich in them has been more dangerous than chrysotile or anthophyllite".

Am I correct that the evidence, the sources of the evidence, to come to whatever inconclusive conclusion you could come to, was first of all Dr. Selikoff's workers, and secondly Dr. Enterline's workers?

THE WITNESS: A. Yes...

Q. With respect to crocidolite?

A. ...and also, I suppose, up to a point...no, I think that's fair. And in none of it was there any quantitative data about dose.

Q. I suppose my question to you is, if you take account of the possible factor of intensity of exposure, which may have been present not only in Dr. Selikoff's workers, but perhaps even in Dr. Enterline's maintenance workers...at least he suggested it may have been...and you take into account Dr. Dement's recent paper, would you still come to the same judgement?

A. I think I would come to the judgement that the evidence is weak, but could be interpreted in that way.

As I said in my introductory remarks, and you quite correctly quoted me, the major part of the case against the amphiboles vis a vis the chrysotile, depends upon the data about mesothelioma. I think the rest of the case, you can take it or leave it, frankly, without affecting the issue very much.

5 Q. I suppose the concern I expressed to you is, given that mesothelioma is a relatively rare disease and given that asbestos-associated lung cancer seems to be a much more common disease, why did the Simpson Committee feel justified in making a distinction in the standard it set among the three fibers, based primarily on the evidence with respect to mesothelioma? If you can argue that there is just as much excess lung cancer produced by chrysotile in certain industrial settings, albeit not others, is there, in your judgement, still a case to make the differentiation?

10 A. Yes. I think, you see, the difficulty about lung cancer is that you've got another factor which is more powerful than asbestos - you've got smoking. One just has not got sufficient information about the smoking habits of these different populations to compare them with one another.

15 You can compare them with the standard at the time, in that particular place, but I come back to the women making gas masks. In World War II, it's likely...though I can't prove it... that the majority of these women were nonsmokers. And yet you've got high relative risks of lung cancer.

20 One is talking about evidence that is an order of magnitude less certain than any evidence about mesothelioma, when one is talking about lung cancer, because of the smoking factor. But on common sense grounds, bearing in mind the points that have been made about mesothelioma, it seems reasonable to conclude that...and prudent to conclude...that fiber type may be relevant, that fiber shape may be relevant to lung cancer as well as to mesothelioma.

25 If you look at the animal work again, the animal work suggests that when you take a fiber of a particular specification, it's related in nonsmoking animals to the incidence of not only mesothelioma, but also of lung cancer.

30 So I would argue by analogy there, and say that, again, in the past, with chrysotile on the whole not having been commercially produced in the fully opened manner, the amphibole

A. (cont'd.) has probably been more dangerous.

Q. I take it from your comments that you have had an opportunity to look at Dr. Dement's report?

A. Yes.

Q. Can I ask you whether, having reviewed his report, it in any way changes the view you took of the situation when you wrote your own report in 1979?

A. Well, I would...the difficulty is that at the moment Dr. Dement's work is in the preliminary report stage, and I think he is going to publish another study in six months time, or soon, which will give us more information about dose.

I think there are a couple of points of interest. One is that in spite of a high relative risk from lung cancer, there is only one mesothelioma, which again suggests that chrysotile and the amphiboles are different in respect of the propensity to produce mesothelioma.

With regard to his relative risk from lung cancer, there are two possibilities. One is that there is something odd about the standard. As you know, and as he will tell you when he comes to give evidence, the counties surrounding the factory and in which the factory is situated have a very high lung cancer rate - seventy-five percent higher than the United States average.

So he was presented with the difficulty as to what was the appropriate standard to take. If you take the whole State of South Carolina, as I understand it it's not very much above the national rate, and he has used that as his comparison and not the local counties.

If he took the local counties as his standard, most of the excess SMR would disappear.

What are the possibilities? One is that there is a factory operating in South Carolina which we don't understand, in those counties, which is a cause of lung cancer, which is totally unknown, and these men in this factory have been exposed to it - something to do with the environment of those counties.

A. (cont'd.) If that is the case, more than the fair share of the effect is being attributed to asbestos, because he is not using the local male population as his standard.

Another possibility, as has been pointed out, is that in those counties during the war there was a big shipbuilding industry. The excess lung cancer rate in those counties, which is seventy-five percent above the national average for the U.S., is due to work in the shipyards, and due to exposure to asbestos in the shipyards, and probably due to a mixture of fiber types.

If that's the case, one wonders how many of the men in the cohort were also exshipyard workers. Twenty thousand men, I understand from the paper, in those counties are known to have worked in the shipyards. It seems very unlikely to me that some proportion of the seven hundred and fifty in Dr. Dement's cohort who, from the material in this study, were old enough to work in the war - indeed he refers to deaths from 1940 onwards - didn't also work in the shipyards, and I think as his study is being used as a source of anxiety in relation to pure chrysotile, I think it is incumbent upon him to make inquiries to determine what proportion of his cohort had in fact worked in the shipyards prior to or subsequent to working in the factory.

Q. If for the moment we accept the excess relative risk that he seems to have produced in his paper, does it...I am going to ask you...does it, in your judgement, make out a better case for industry-by-industry regulation?

A. Yes, that's a different point. I mean, should one do something about textiles separate from brake linings?

Q. Precisely.

A. Well, I think it would. If you take the result at its face value and it's not to do with a local factor and previous work in another asbestos-related industry, it suggests that the work in that factory was particularly dangerous...that's certainly true...which we know was making textiles.

Q. When you came to develop your range of options for the Committee, I take it that the studies upon which it was based are all set out in your table thirty-five?

A. Yes.

Q. At page seventy-five?

A. That is correct. Those were the only studies in which, as far as lung cancer was concerned, at that time there was information which could be related to the current standard in fibers per c.c.. by transforming the data, of course, from dust particles.

Q. So you took the McDonald work in Quebec, Enterline's work and the work at Rochdale?

A. That is correct.

Q. All right. In respect of the work at Rochdale was it Peto's work or Berry's work?

A. Richard Peto's work on lung cancer.

Q. In selecting those studies...

A. I'm sorry. Julian Peto's.

Q. In selecting those studies were there any other criteria that you applied to select the studies in the first place, other than the fact that they had quantitative assessments associated with them?

A. None whatever. If there had been any other data in which it was possible to make some sort of estimate of dust, we would have used it. But there were none that we found.

Q. Can I ask you this, if for example Dement's paper had been available to you in 1979, would it have found its way onto table thirty-five?

A. It certainly would have done, provided the hygienists were satisfied that the dust levels approximated to the truth. I'm not suggesting for a moment that anything has been falsified, but that the dust levels were acceptable and also, of course, we would have had to have taken into account any conversion factor that he used. I have referred to a different

A. (cont'd.) situation here where Dr. Dement uses a very extreme conversion factor, from nanograms to fibers.

Q. In relation to...in nonoccupational settings?

5 A. Yes, and I would be very interested to know what transformation factor, conversion factor he has used or is using in his work in North Carolina, or South Carolina.

Of course there is another study, as is so often the case, another group of workers is studying the same factory - namely, Dr. Alison McDonald. So that we would also have used her data.

10 Q. May I ask you whether, were you doing the report again in 1981, what other studies would have found their way onto table thirty-five?

A. The Berry and Skidmore study, if there was information about fibers. I don't know of any others.

15 Q. The Berry...is this the friction materials...

A. Ferrodo, yes.

Q. ...plant?

A. Yes.

Q. All right.

20 A. It's interesting that two studies relating to chrysotile alone have been published since this report, one which suggests that there is no risk associated with exposure to chrysotile...

Q. That's Berry?

25 A. That's Berry. And the other which suggests that there is a high risk of lung cancer. So you've got, in fact, as it were...if they were added to this table, one would be at the bottom and one would be at the top.

Q. Table thirty-five and the studies therein... let me approach it another way.

30 Dr. Selikoff's work, I take it, was not on table thirty-five because there were no quantitative measurements...

A. Exactly.

Q. ...of dose?

A. Exactly.

Q. What about Dr. Weill's study?

A. Dr. Weill's study was published after we went to press, and his study was also...I had quite forgotten that... his study would have also been used, although we had some misgivings about the followup rate.

Q. In terms of the large percentage unaccounted for?

A. Exactly.

Q. Having put the studies on table thirty-five from which you developed your range of options, did you for the purpose of developing that range of options accept the results in the studies at their face value?

In other words, to give you an example, we have heard, for example, that Dr. Enterline's study may underestimate the risk because he has got a population of survivors.

I note you made that point in the text. I'm just wondering in terms of developing your range of options whether you accepted all of the results at face value or whether you attempted to re-evaluate them in terms of any methodological problems?

A. When we interpreted the data to the Committee, we took into account these points. I think it's set out in paragraph 258. "In choosing a figure from table thirty-five, it should be borne in mind that there are a number of identifiable factors in the various estimates which exert opposite effects. The linear hypothesis may overestimate the risk, but the Quebec data, because they are derived from mines and mills, and Enterline's data because they are restricted to men who survived long enough to reach pensionable age, may underestimate the average risk

A. (cont'd.) "to men exposed for the whole of a working life to more disbursed fiber in process work.

A further conservative assumption arising from the use of the concept of accumulative dose, in table thirty-five, is that the biological effect of asbestos is assumed to be immediate and instantaneous. The residence time of a fiber in the lung is ignored, and no allowance is made for a continuing action of fibers."

And then there is this point about Rochdale, which really is telling us that...the estimates of the dust there, again, are subject to controversy, as is the nature of the fiber used, and we really couldn't, in the end, get to the bottom of what was happening there.

Then we come to a conclusion...at the end of 259... "A two percent excess mortality would be occurred in association with the levels recorded in the penultimate column in table thirty-five. These levels range from five fibers per c.c. to zero point four fibers per c.c. In other words, a spread of times eight - an eightfold spread. Bearing in mind the points made in the previous paragraphs, a figure towards the lower end of this array might represent a compromise between what are, to be truthful, very considerable uncertainties." I would be very happy to rest on that one.

MR. LASKIN: Perhaps it's a good time to take a lunch break, Dr. Acheson.

DR. DUPRE: Shall we rise, then, until 2:15?

MR. LASKIN: Sure.

THE INQUIRY RECESSED

THE INQUIRY RESUMED

DR. DUPRE: Very well, counsel, are you ready?

MR. LASKIN: Sure.

MR. LASKIN: Q. I just come back to table thirty-five for a moment, Dr. Acheson, which is at page seventy-five of your report.

THE WITNESS: A. Yes.

Q. I take it this summarized the quantitative risk assessment that your group did, is that accurate?

A. That's in relation to lung cancer, yes.

Q. Was there any other quantitative risk assessment done in the report?

A. We had to deal with the other matters in relation to semi-quantitative data.

Q. When you say semi-quantitative, do I take it by that you mean you have some indices such as length of exposure...

A. Exactly.

Q. ...but you don't have actual dust or fiber measurements?

A. Precisely. Precisely, yes.

Q. Now, when you developed your range of options, I take it from what you have said and from table thirty-five, that the index that you did use was mortality in relation to lung cancer?

You are going to have to say something for the court reporter over there.

A. Yes. Sorry.

Q. Can I ask you this, I noted when the British developed their original standard in 1968, the index that was used, if I'm not mistaken, was a morbidity index or prevalence of rales or crepitations. May I ask you whether there was...whether it was a conscious and deliberate decision to move away from a morbidity index to a mortality index?

A. No, I don't think so. And of course you have corrected me in a sense, because we did also look at quantitative data in relation to asbestosis, and it is discussed in the report.

5 Q. Dr. Berry's work?

A. Indeed. In 1968, the only quantitative information available was relating to asbestosis in the original publication of the Rochdale material, so that I think it's fair to say that we took into account both, but felt that the mortality data was, in relation to lung cancer, was probably more important for the future than the asbestosis data.

10 Q. A couple of our witnesses who have been here before...in fact Mr. Berry was one of them, and Dr. Enterline more directly...made the point that in terms of the standards that are now being set - two fibers or whatever - it's not going to be possible epidemiologically to test whether those standards are producing excess deaths because essentially you aren't going to have a large enough population to be able to measure or detect a risk. One of the things that was suggested is that perhaps we should move away from a mortality index to some other index which would enable us to measure or detect risk at the levels that we now have and with the cohort populations that we are studying.

15 Q. Could I ask you whether that kind of thinking was brought into play and was it considered?

A. It certainly wasn't considered in the report. One of the things that I learned in relation to my work with this Commission was how difficult and contentious the whole field of the definition of diagnosis of asbestosis is. We had experts whose particular expertise was the diagnosis and treatment of asbestosis, on our Committee, including Dr. John Gilson and Dr. Margaret Turner-Warwick. Having come from a field where I was aware of the difficulties of attributing deaths to cancer and other things, I was relieved to find that the difficulties associated with the diagnosis of asbestosis are much greater, and

5 A. (cont'd.) I don't think that any useful purpose is likely to be served in the foreseeable future in trying to define an asbestos standard in relation to some early sign or physiological test for asbestosis, frankly.

I speak as someone who is more a spectator in that particular field, but having seen the experts disagree on these matters of definition, and diagnosis of asbestosis, that is my view.

10 Furthermore, of course, you have precisely the same problems of lead time in relation to asbestosis that you do in terms of cancer. And I don't think again that I can see any way in which it's likely to be able to test the standard, be it two fibers or one fiber or whatever, in the short term in relation to some physiological test of lung function.

15 Q. Dealing with table thirty-five, I take it that one of the assumptions that is in table thirty-five is a linear dose-response relationship...

A. Yes, indeed.

Q. ...at all levels?

A. Correct.

20 Q. Through to the zero intercept?

A. That is correct.

Q. The second matter I noted in your report was that you assume that about half of the mortality attributable to chrysotile would be due to lung cancer, and about half to asbestosis?

25 A. Had been due to.

Q. Had been?

A. Yes.

Q. Can you...where did that come from? I had trouble finding it, in terms of the data. Is it in table eleven?

30 A. I think the best way I can help the Commission in answering that question would be to take it away and write to you. I wouldn't like to go on the record because I'm uncertain,

A. (cont'd.) frankly.

Q. That's fair.

A. Would that be helpful?

Q. Sure.

A. I'll make a note of it.

MR. LASKIN: If we get some communication, I think, Mr. Chairman, we can certainly distribute it to everybody and it would be part of the public record.

DR. DUPRE: It would be greatly appreciated, Dr. Acheson.

Could I just ask a small question pursuing counsel's line of questioning here?

MR. LASKIN: Sure.

DR. DUPRE: With respect to the quantitative risk assessment, insofar as morbidity and asbestosis are concerned, is table twenty-two the table that offers your quantitative risk assessments? It deals with Rochdale...

THE WITNESS: Yes, that is correct. Table twenty-two, and there is a figure which goes with it, which is figure seven on page seventy-nine...which sets out in figurative terms three lines relating to three definitions of asbestosis - crepitations, possible asbestosis and certified asbestosis.

The history of this study is that the sample of persons who were examined in 1968 were persons currently employed at Rochdale, and did not take account of people who had left or died due to, for example, asbestosis.

In my opinion, that was a serious error of design of the original study, and the subsequent one which this reports took account of this, and included in the assessment of end result, the state of a substantial proportion of those who had left.

When this was taken into account, it certainly appeared...and it appears if you look at this figure, figure seven,

5 THE WITNESS: (cont'd.) that some substantial proportion of people exposed to a hundred fibers cumulative over fifty years, that would be the present standard, two fibers for fifty years, do indeed develop asbestosis, and we set out in the text proportion, which I won't quote without actually reading it, but it is in the text.

10 However, that taken at its face value is very alarming, but if you take into account the de facto tightening of the standard that has taken place between 1968 and 1978, and accept a factor of, I think five, as appropriate, most of this disappears and one is left with the proposition that the standard of two fibers as currently defined is not likely to be associated over a lifetime with an appreciable amount of asbestosis. So one gets back, as it were, to square one, if you take into account the de facto tightening.

15 The whole question of whether one accepts and to what extent one accepts that there has been a tightening of the standard, of course, is a matter on which there is some difference of opinion.

20 MR. LASKIN: Q. There is a difference of opinion on that question?

25 THE WITNESS: A. Amongst the experts. The Committee was prepared to accept that there had been...the Simpson Committee, I think, was prepared to accept that there had been an approximately fivefold increase in rigorousness of the standard.

Q. Is that based on Mr. Steel's calculations...

A. Yes, that is right.

Q. ...which I see on table thirty-seven?

A. That is correct.

30 Q. So that he attributed modern counting techniques, which I take it is graticule as opposed to full view?

A. And then the difference between personal sampling and...

Q. And gave them each a factor?

A. That's right.

5 Q. I don't want to pursue this with you because I know you didn't prepare it, but just for the record, the more detailed calculations that led to table thirty-five are, I take it, found in appendix six, which is at page sixty-two?

A. That is correct.

10 Q. That, as I understand, was prepared by your statistician who worked with you, Mr. Gardner?

A. That is correct.

15 Q. Now, with Dr. Enterline's work and with Dr. McDonald's work, I take it you took the dust particle measurements and applied various conversion factors?

A. That is correct, and they are shown. The ones that we selected...the range which we selected are shown in table thirty-six and thirty-five.

20 Q. I noted the figure two came from the conversion factor that was before the Beaudry Commission, as I read your report?

A. Yes, sir.

25 Q. Was there any source of the conversion factor five?

A. None. It was simply considered by us to be a reasonable top number, on the basis principally of what some people would refer to as common sense, and others as guess work, I suppose.

30 Q. What about one at the other end of the spectrum?

A. Again, that seemed to be the view of the Committee and Steel, that that would be a reasonable lower limit to consider, that any factor below one would not be a reasonable one to consider.

Q. With respect to Rochdale, the conversion factors there relate to these different methods of measurement

Q. (cont'd.) that you told me about?

5 A. Indeed. I think it is important to point out, however, that there is a concealed conversion factor which we do not know.

Q. In Rochdale?

10 A. In Rochdale, as in any other historical industrial data relating to asbestos. All counts prior to 1960 were made of total dust particles because the fiber method was not used anywhere in the world, so therefore, in relation to Rochdale as elsewhere, a conversion factor has had to be made by someone at some stage, when they refer to information which was recorded in relation to measurements taken before 1960.

15 I am not aware that the actual conversion factors used have ever been published, but I may be incorrect. Mr. Julian Peto will tell you, no doubt, if you ask him.

20 Q. I take it that when you wrote the report that you came to the conclusion that however uncertain these conversion factors were and however unreliable they were, they were better than nothing or they were better than simply taking length of employment, duration of exposure. Is that fair?

25 A. I think they are better than...they are more serviceable than taking length of employment, because you can relate them to the present standard, which is in fibers and not in a preferred period of employment, so therefore there is that advantage to it.

30 How you decide between...within the range that we took as a range which the Committee should consider, of five fibers to point four, is really a matter of judgement...or guesswork.

Q. Does your judgement still stand today on the value of at least trying this exercise? That is, trying to convert from dust measurements to particle measurements?

30 A. Yes. I think one has to try to assess and evaluate the work of scientists who have struggled with the data.

A. (cont'd.) In the end, it leaves such a margin that in effect it may be that it hasn't exerted a great influence in the final decision, I would suspect.

5 But nevertheless, I think it is an exercise which had to be carried out.

Q. Just looking at table thirty-five for a moment, just to take one example so that we are all clear on how that works, if you look at Quebec and if you use a conversion factor of two, do I then take it that a standard, a two fiber standard will
10 produce one percent excess lung cancer?

A. Mortality, if each man is exposed - two fibers, eight hours a day, five days a week for fifty years.

Q. For fifty years.

A. As I pointed out this morning, if you actually set up a standard of two fibers, perhaps a tenth of the men will
15 be so exposed, or perhaps a proportion which will depend upon the particular operation that is being carried out in the factory.

But by no possible construction of the data will they all be exposed to that level, so that in effect if you set your standard at two on the understanding...according to the
20 assumption of a one percent excess mortality, the excess mortality will be substantially less.

Q. Also then implicit, if we take the same example, is that excess mortality for all asbestos-related disease, for men, ideally, who work for fifty years, five days a week, eight hours a day, will be two percent?

25 A. If...yes, I am fairly confident that this estimate that the proportion attributable to cancer is half the total, is based on a summation of previous experience, and I will write to you on that point.

If it is the case, it may, in the future, overestimate the situation because it seems generally agreed that
30 mortality associated with asbestosis has been associated with

5 A. (cont'd.) high doses, and it's unlikely that the same proportion of mortality will be associated with doses which are received if a two fiber standard, or a one fiber standard, some low standard, is adhered to.

Q. You really anticipated my question, but to the extent that lung cancer occupies a greater proportion of the total of excess deaths attributable to asbestos, in terms of the approach of the Simpson Committee, these would be overestimates, looking at the whole picture?

10 A. Yes. There is another assumption, and that is that public habits with regard to smoking tobacco will not change. Actually, it seems likely that changes in public attitudes, public habits, changes in tobacco habits, will influence the excess mortality to a greater extent than changing or reducing the asbestos standard - in relation to lung cancer, but not mesothelioma.

DR. UFFEN: I want to make sure that I understood. Just back up a sentence or two.

20 When we were talking about the two percent fiber example, if that is the standard, are the...do the regulations allow excursions above and below that two percent?

THE WITNESS: They...

DR. UFFEN: Is that an average?

25 THE WITNESS: The standard is the maximum which is permissible over a four hour average period, but everyone is enjoined to remember that it's a maximum and that every effort must be made to keep the dusts as low as possible within the standard.

DR. UFFEN: What I'm getting at is, in that four hour averaging period, which averages out to less than two, there could be fifteen minute excursions of as high as ten...

30 THE WITNESS: I believe that is...

DR. UFFEN: ..and down to..as long as it averaged out?

THE WITNESS: Certainly they could be higher, yes.

DR. UFFEN: They could be higher?

THE WITNESS: That is true.

5 But in a factory in which the standard to which they have to adhere is two fibers, the majority of the measurements will be less than two fibers.

10 In fact, if you look at volume one of the report you will see the levels that the factory inspectorate has found in England since 1968...if I can find it.

Some of it is in figure one on page seventeen.

DR. UFFEN: Is this in volume one?

THE WITNESS: Volume one. You can see that..

15 "Distribution of asbestos dust exposure levels in different manufacturing industries in Britain, 1972 to 1978, four hour personal sampling data," page seventeen..."in the asbestos-cement industry, ninety-eight point five percent of the results were under two; millboard paper, ninety-nine point six; friction materials, ninety-five percent; textiles, ninety-five percent, and insulation board, eighty-eight point six."

20 So that in the very active maintaining, trying to keep within a standard, it is inevitable that most of the measurements will be below the standard. Therefore, most of the men will not be exposed to the level which attracts an excess mortality such as has been calculated in table thirty-five.

25 DR. UFFEN: What was in the back of my mind was the question that has come up once or twice with previous experts about the possible importance of short, but intense, exposures. In a four hour interval of the man working where something goes on for twenty-five minutes and he really gets a big exposure, and then he stops work for the remaining three and
30 and three-quarter hours, so his time weighted average on his personal sampler shows an acceptable level?

THE WITNESS: Yes.

DR. UFFEN: This is a remote possibility?

THE WITNESS: We considered this in the Committee
5 and we were advised that there is no scientific evidence which
enables one to distinguish between the effects of intermittent
and continuous exposures.

The only possible area where it could be argued
that there is some evidence in man is in the observation that
10 often maintenance workers have higher morbidity and mortality
than process workers, and it has been argued from that that
intermittent exposure may be worse than continuous exposure...
also the experience of pipe fitters.

However, it can equally well be argued that it
is not that their intermittent exposure has been the dangerous
15 factor, it has been the fact that these have been very dusty
jobs and that the total amount of dust to which they have been
exposed is greater than the process worker.

So far as I know, there is no way in which one
can distinguish between these two confounding factors.

MR. LASKIN: Sorry.

DR. UFFEN: Could I ask just one more question?

MR. LASKIN: Sure.

DR. UFFEN: A hypothetical case where a worker
does get involved in a place, and he's got a personal monitor,
and he knows that he has suddenly been exposed - a big chunk
25 just fell on his head - is it any use his stopping work until
his average has gone down?

THE WITNESS: I know of no information which helps
guide in what he should do - no scientific information.

MR. LASKIN: Q. Just pursuing that, and you may
have answered it, but just pursuing that line for a moment, does
30 your answer that there is no scientific evidence one way or the
other extend to this hypothetical case: a person who, say, gets

5 Q. (cont'd.) a certain dose in two years, as opposed to a person who gets the same dose in ten years? No intermittency, it may be continuous, but one person is exposed to a much more intense dose over a shorter time period?

THE WITNESS: A. Again, I'm not aware of any evidence which enables us to distinguish the effects, in epidemiological terms.

10 Q. What about in medical terms? Some persons have suggested before us that the lung may have a clearance mechanism that may or may not get overwhelmed, depending upon the intensity of the burst. Did that kind of issue come before the Committee or the working group?

15 A. Yes, it was certainly discussed. The chest physicians on the Committee referred to this possibility that there was a cleaning mechanism which could break down in a position where there was an excessive dose.

But this was never taken into account in any recommendations with regard to public policy...to my knowledge.

20 Q. Do I take it from that the Committee could not come to any judgement as to its medical or scientific validity?

A. Exactly, exactly.

25 Q. Before we leave table thirty-five, I note that this is a range of options, and I note in your report you were careful not to express any opinion as to what the standard ought to be. Would it, nonetheless, be fair to ask you, putting on your other hat - that is your hat as a member of the Simpson Committee itself - whether you were in agreement with the recommendation that was ultimately proposed with respect to standards?

30 A. Yes, indeed. It was a unanimous decision. There was nobody who had major reservations about our proposal for a differential, a chrysotile standard of one, an amosite of

A. (cont'd.) point five, and crocidolite of point two, if I recollect correctly, with a prohibition of importation of crocidolite.

5 But as I said before, one of the sort of half the argument on which the eventual recommendations were made was never tested.

10 We were subject to very close scrutiny in the opinions that we had expressed about the reliability of the sort of extrapolations that we made here, and whether we ought to have undertaken these or...and the whole question that we discussed this morning of the lack of agreement between the animal data about fiber type, and the human data, and so on and so forth, but the whole question of the ability of the industry, economically and technologically, to face this range of options, and the whole question of which substances require the use of this, of 15 asbestos, as opposed to some other substitute, was never tested in the same way, and I regard that, in retrospect, as a deficiency of the work prior to reaching the judgement that we did, which I hope subsequent committees addressing the same problem will deal with in a different way.

20 Q. How, as a practical matter in the workplace... if you have an establishment that is working with more than one fiber type...let's say chrysotile and amosite...how as a practical matter does that workplace regulate other than below point five?

25 In other words, is it possible in a mixed-fiber workplace to regulate one substance at one level and another substance at another level, or must you really regulate or control to the tightest standard?

30 A. My reading of the situation is that there is no way in which you can have a different standard for different types of fiber in the same workplace. It would have to be to the lower level, in the two situations where amosite is used in

A. (cont'd.) the United Kingdom.

5 Q. So that is what follows from that that in any plant where amosite is used, regardless of what else is used alone or in conjunction, you in effect are requiring a standard of point five?

10 A. That is certainly the case with insulation board manufacture. I feel sure that you would receive more expert testimony than from me about what happens in pressure piping. Although I have been to a pressure piping factory in Canada, from memory I cannot recollect whether the amosite was used alone or in conjunction with chrysotile.

15 I think in the United Kingdom, you see, the only situation where both are used is insulation board...or what were used...and that is no longer the case. Amosite is used without chrysotile now, in the United Kingdom, in the two factories where they make insulation board.

I think that it may be that in making pressure piping, amosite is the only fiber used, and chrysotile is not. So that it may be that this does not arise, in practical terms.

20 But if it did arise, I feel sure that you would have to control to the lower.

25 Q. Just one further question on your judgement as to the standard. May I ask you at a distance of some two years from writing your report and in light of the subsequent epidemiological and medical evidence which you have already told us about, whether your judgement has changed in any respect?

A. No, as a matter of fact it hasn't. I think that the evidence that has been published since 1979 strengthens the case that in the...as far as the human epidemiological evidence is concerned...the amphiboles are more dangerous than chrysotile.

30 Q. What about in terms of the actual level that was recommended by the Simpson Committee?

5 A. I have heard nothing which makes me change my view that the one fiber standard is a reasonable standard for chrysotile, and a standard which the industry can adhere to without difficulty.

10 Q. May I ask you about one other theory that has been put before us during the course of these hearings, and it was first put forward by Dr. Weill. The theory was this, that if you set a standard that is low enough to protect against carcinogenic risk, you will also be setting a standard low enough to protect against fibrogenic risk...or to put it the other way around, if you protect against any excess evidence of asbestosis, you will also be protecting against any excess cancer.

15 Did that hypothesis, did that thinking come before the Committee?

20 A. No, I don't think we discussed that. I think the Committee was of the opinion that asbestosis, chemical asbestosis, in terms of restriction of capacity to breathe rather than any minor appearance on the x-ray plate, and death, is associated generally with high doses of asbestos, so that in that sense if you succeed in controlling the cancer risk you will control the asbestosis risk.

25 But I don't think we know enough about the dose-response relationships for mesothelioma to know whether that's true for mesothelioma and the amphiboles.

30 Q. Fair enough.

Can I just ask you one or two questions about dose, and the calculation of dose, and I take it from the report that the calculation figure that you used was cumulative dose?

A. Right.

35 Q. When Mr. Berry testified before us, and I'm sure you are aware of them, he put forward a number of different hypotheses based upon fibers remaining in the lungs with various assumptions about elimination, and so on. Were those kinds of

Q. (cont'd.) hypotheses before the Committee?

5 A. The Berry paper was discussed, and indeed his material...he gave evidence to us in person and his paper referring to the different types of analyses was certainly discussed by the Committee. But it was felt that the only practical way in which one could use the data that were available was on the basis of cumulative dose, because that was the unit in which it was recorded. We had already decided that there was more data about mortality than morbidity, and that the public issues which had to be looked at were, the greater public issues which had to be dealt with were the cancer risks than the asbestosis risk, which it seemed, on the face of it, was likely to be more or less controlled by a two fiber standard. So therefore, we didn't feel that there was any need, or indeed was it possible to use his other approaches, although we did take care to point out in the report that our work was based on the assumption that there was no substantial long-term or delayed hazard associated with the presence of fibers in the lung...which is probably untrue.

10 Q. Because when you measure your cumulative dose, you assume that the dose comes in and instantly leaves?

20 A. That's right.

Q. I take it you also assume that there is no change once exposure ceases?

A. Quite so.

25 Q. Is what you are suggesting that that's the data that we've got, but they may not...those assumptions may not be medically or biologically realistic?

A. That is correct. One is giving the same weight to an exposure of five fibers for two months ten years ago, as to an exposure of two fibers for five months one year ago.

30 Q. Did you conversely find any medical or biological support for Mr. Berry's thesis?

A. No.

5 Q. Would I be correct that in terms of which approach produces a more conservative estimate, the approach based on cumulative dose, I take it, is by and large likely to produce a more conservative estimate, is more likely to overestimate the risk? Is that correct?

A. I think that's correct.

10 Q. I suppose one of the issues that ultimately I'm trying to ask about is whether you can separate out the concentration of dose that one gets from the duration of exposure, whether you can adequately sort out the effect that those two factors may have?

A. I'm not sure that I follow you, honestly.

15 Q. One of the propositions or one of the difficulties, as I understood it, with measuring in terms of cumulative dose, for example, is that you can't adequately separate out whether you are seeing the effect of increasing dose or whether you are seeing simply the effect of duration...?

A. That's quite so. Quite so.

20 Q. Did your medical group attempt to address that difficulty in any detail to see whether you could sort out what the effects of the two were?

25 A. The answer is no. I think we had in mind that many of the cohort studies do not deal with what effectively are three confounding variables, including the one of time period which the exposure took place, duration of exposure, and latent period... those three things. But I'm not certain how that would affect the practical issue of setting a standard, in the light of the data that are available.

30 We certainly did not consider at all any, coming to any recommendations about duration of employment, making any recommendation that an asbestos worker should work such and such a time and not another time.

In any event, if you assume a linear hypothesis

5 A. (cont'd.) increasing the number of persons exposed and reducing the time of exposure will not diminish the yield of attributable cancers. You will have exactly the same number of cancers, but they will fall in different thoracic cavities...different people will get them.

Q. You've mentioned a couple of times today the evidence that was before you on the fiber types that were found in the lung...

A. Yes.

10 Q. ...on autopsy or post mortem. What conclusions did your Committee draw about all of that evidence? Is it helpful in terms of public policy, in your judgement?

15 A. I think the evidence that was available when we reported was pretty defective in terms of fiber type. It was about, principally about asbestos bodies, and it showed that very substantial proportions and increasing proportions over the last thirty years of the population have an asbestos burden in the lungs. Yet, they do not seem to get asbestosis unless they have worked in the industry.

20 That was taken as indicating that it was probably a threshold in terms of the development of asbestosis. I don't think any other conclusions were drawn about the question of a public health risk of lung cancer associated with asbestosis, from those data. Our conclusions on that score were based on extrapolation backwards from the industrial data.

25 I think the data that I referred to this morning has been published subsequent to the report and gives detail about the nature of the fiber in cases and controls, and I think that's been a valuable addition to the literature.

30 Q. Early on in your...I'm switching topics on you now, but early on in your report at paragraph seven, after dealing with...which is at page nine...after dealing with the biophysics of asbestos and so on, you expressed the concern about

5 Q. (cont'd.) current optical methods of measurement. Now I know your Committee has looked at this, and did they relook at the question at the time you prepared the report, as to whether they should maintain optical microscope measurements in the workplace?

10 A. My recollection, and I honestly haven't thought about this or prepared anything about it, is that there is a list of recommendations somewhere about future research. It was certainly intended that there should be. I wonder is it a...and one of them was that there should be further studies to develop better measures of asbestos in the workplace.

I'm sorry, I can't put my hands on that.

15 Q. It's all right. We can find it later.

I take it the judgement of the advisory committee at the present time is that it should continue with optical microscope measurements in the workplace?

20 A. For the time being. For the time being, yes. But that further research should attempt to develop better measures.

25 Q. Just a few other isolated questions. Was there any evidence before your Committee as to the possible exposure from asbestiform substances in known asbestos mining situations, for example, which is a subject that has come to our attention here?

30 A. This was discussed and considered, and it was concluded...and I think it is stated in the report...that it was the opinion of the Committee that the vast proportion of exposure to fibrous minerals in the United Kingdom came from commercial asbestos and not from fibrous clays and other minerals of borderline commercial use. But of course there has been the interest which has arisen in relation to zeolite, etc., has largely risen since the report was published.

However, I'm not aware of any evidence that there has been any appreciable exposure in the United Kingdom, at any rate,

A. (cont'd.) to other fibrous minerals, so far.

Q. Then just one or two questions about gastrointestinal cancer, which you mentioned this morning.

A. Yes.

Q. There are really two questions. The first one, in your subsequent article which you gave at the Lyon Conference, you raised the possibility...and I think it's at tab eight, page 748, you raised the possibility of another factor at work in producing that kind of cancer. Have you any speculation as to what that factor may be?

A. No, but I think that...I still think that there is likely to be another factor, because of the irregular way in which these excesses of cancers of the alimentary tract appear. They are found in some studies, and not others. They are found almost exclusively in studies where there has been heavy exposure to amphiboles, and they are usually associated with excesses of peritoneal mesothelioma as well, as I think we have demonstrated in one of the tables where we set out...table three A, on page 65 of volume two of the report...where we set out the excess in rank order, the observed over expected ratio for gastrointestinal tract, and the number of peritoneal mesotheliomas. There is a rough relationship, as you can see.

We also have made the point on a number of occasions that the alimentary tract cancers, coming back to something that Dr. Mustard said this morning, have not really been tested in terms of precisely what tumors they were. Looking at Dr. Selikoff's papers, in most of them, at least those that were published when we wrote this report, the death certificate diagnosis of esophageal, stomach, large intestinal cancer, had been accepted without histological examination, and I think that before one attributes an additional anatomical site of cancer to an industrial origin, it is highly desirable that some sample of the cancer should be examined by a pathologist, particularly when there is the

A. (cont'd.) possibility of confusion with peritoneal mesothelioma.

5 I think that should be done before anyone assumes that asbestos always...high doses of asbestos are always associated with an excess of cancer of the alimentary tract.

Q. Just coming back to page 748 of that article.

A. Yes.

10 Q. Right at the very bottom, you speak more generally of unknown factors may be operating in different conditions of the workplace, and I take it you are there referring to the fact that that may be one possible explanation for the kinds of different results that we see in various work settings?

A. Yes.

15 Q. Can you tell us what any of those factors may be? Is there any speculation from anywhere as to what factors may be at work?

20 A. I don't know, but one has to bear in mind that people working with insulation materials are working with a wide range of materials, of which asbestos is only one. They are using calcium silicate and all sorts of other things at different times have been added into the sort of material that pipe fitters use, and it's conceivable that one or other of these factors may interact with asbestos, but I know of no direct evidence. What one is doing here is trying to find an explanation for the fact that in certain cohorts one finds a really quite large excess...apparent excess...of gastrointestinal tumors, and in others one doesn't.

25 In Britain, the Rochdale cohort is associated with substantial excesses of alleged alimentary tract cancers.

Did I say Rochdale?

Q. Yes.

30 A. I meant Barking. Sorry, Barking. Rochdale is not. There is no excess with Rochdale.

Q. The other question specifically about gastrointestinal cancer relates to your paragraph 215 in your report.

A. Yes.

5 Q. Conclusion two, you raise the possibility that there may be some threshold, safe threshold in respect of that type of cancer. What's the evidence that supports that, the fact that you get these irregular results?

A. There are two bits of evidence. One is that the industrial populations where there has been an excess of alimentary tract cancer are all industrial populations with very heavy exposures...often associated with the insulated business.

10 The second point is that the only dose-response relationship that has ever been drawn comes from Quebec, and it can be interpreted by eye as indicating that there is a threshold, but I really wouldn't want to put much weight on that.

15 I can find it, I hope. It's figure fifteen and sixteen, page eighty-two. If you look at fifteen, there is a clear dose-response relationship for respiratory cancer. It's possible to interpret the dotted lines, which are alimentary tract cancer, as indicating no increase in risk up to a dose of three hundred millions particles per cubic...some point between two hundred and four hundred, where it begins to go up.

20 I don't think Dr. Enterline's work helps us very much. There is a clear dose-response relationship with respiratory cancer. The digestive ones, I think, are very difficult to interpret. You could say there was no dose-response relationship there.

25 So I wouldn't like to place much reliance on this aspect of the published work, because the quantitative data is very slim.

Q. Is there anything that has happened since 1979 that casts any light on that?

30 A. We have some additional studies published, none of which, to my knowledge, show any excess in alimentary tract cancer, including the friction material study in England, the Ferrod study. None of the three cohorts Dr. McDonald is following in

A. (cont'd.) the United States, as far as I know, show that if anything the proportion of industrial cohorts showing a positive result is smaller than it was before.

5 Q. Your own work in respect of the two plants in the United Kingdom that you spoke about, has it yet reached the stage where you are able to comment on what you have found?

A. The answer to that question is no, I'm afraid. We have promised that the first people to know the results will be the Health and Safety Commission who commissioned the studies, and the industry and work force who we are studying, but I would be very disappointed if the data were not available for you this year... if you wish to see it.

10 Q. That would be most helpful.

15 Just one or two final questions about general environment. I made a note when you spoke this morning about your comments concerning the health effect in respect of buildings, and I noted that you said...I have it down that you felt the danger was quite small, with one or two important provisos.

20 May I ask you what those one or two important provisos are?

A. They relate to buildings in which amphibole asbestos has been used as a spray, and is therefore available for erosion, and if you look at table thirty-four, you will see the assessment of asbestos materials in buildings, which are susceptible to damage according to evidence of the Royal Institute of Chartered Surveyors. You can see high asbestos spray to walls, steelwork, pipes, preformed asbestos insulation, pipes, machinery, etc.

25 Now, there is a published paper which indicates that some naval storerooms in the United Kingdom were found to have levels of ambient...or crocidolite in the ambient air which were higher than the then standard. There is a reference to that somewhere.

A. (cont'd.) Harries has described a naval store in which the level of crocidolite exceeded the standard. That's page forty-two, paragraph 220, it's the last line.

5 Young reports an elementary school in the United States where asbestos fell from the ceiling onto the furniture, and personal samplers attached to children's clothes showed a concentration of up to three point eight fibers per c.c.

Q. Do you restrict your provisos to amphiboles?

10 A. Well, I think that it would be prudent to consider chrysotile as well, but I honestly think that, bearing in mind the points we have raised this morning, the amphiboles, in my opinion, are more dangerous - certainly in relation to mesothelioma, and perhaps in relation to the others.

15 Now, my other proviso was not in relation to buildings, but was in relation to dumps. I think that exposed dumps of asbestos waste and tailings are a menace...if they are anywhere near human habitation, and there is a tendency as our urban society extends for areas which were previously thought to be quite appropriate to dump things to become areas where new housing estates are built. We have had occasion within the last
20 two or three years in the United Kingdom to have problems with dumps of asbestos, both in exposed areas and also people digging it up in their gardens, where it had been buried. So I think there is a special problem there and it has been shown that uncovered asbestos waste dumps can be associated with very high
25 levels in the immediate area.

MR. LASKIN: Mr. Chairman, I think I've probably kept the witness much longer than I should have, so I'm happy to turn him over to my friends.

He may wish a brief recess. I don't know.

30 DR. DUPRE: Would the parties like to recess now to assemble a batting order, or would one of the parties like to address a line of questioning now, and the afternoon will be

Acheson, cr-ex

DR. DUPRE: (cont'd.) broken up a little more evenly?
I'm in your hands.

MR. HARDY: Let's take a break now.

DR. DUPRE: All right. We'll rise until, say,
quarter to.

THE INQUIRY RECESSED

THE INQUIRY RESUMED

MR. LASKIN: Mr. Chairman, before I turn Dr. Acheson over to my friends, I know he had certain conclusions which he has prepared which he would like to put before this Commission, and it might be the most appropriate time to do that right now.

DR. DUPRE: If you please, Dr. Acheson.

THE WITNESS: Thank you, Chairman.

I should say that these are personal conclusions and in no sense represent any change in the view of the Simpson Committee. The Simpson Committee is no longer sitting and its report is published and it has no opportunity to change its opinions, even if it wanted to.

But I, thanks to your invitation, have had the opportunity to come back to the report and look at it again in the light of subsequent thought and subsequent publications of evidence, and I took the opportunity yesterday to just write down my own personal conclusion as a result of the experience that I have had since the Committee completed its work.

These conclusions are as follows:

One: And they should all be preceded by the words 'in my opinion'...the object of a prudent society should be to limit the use of asbestos as soon as is practicable to situations where it is either essential - that is to say there is no substitute - or it confers a substantially greater benefit than risk.

5 THE WITNESS: (cont'd.) Two: In every situation where asbestos is used, it should be incumbent upon those responsible for its use to consider whether a substitute is available. For example, for such uses as fire resistant textiles, friction materials, asbestos cement and in particular the special case of pressure piping and insulation board. I have given those examples as the examples which I believe to be the situations where there is the strongest case for its use. There are many other uses, but it seems to me that the case for substitutes is much stronger in the others than in these.

10 Three: Where asbestos is used, the object should be to reduce exposure to levels which are reasonably practicable in the light of current technology and costs.

15 I envisage, therefore, a phase reduction in levels as technology advances. I regard the proposals recommended in the Simpson Report as a first step in the direction of further reductions as technology advances.

20 Four: In view of the greater risk in man, of mesothelioma associated to amphiboles than to chrysotile, more sever controls should be introduced for the former than the latter, forthwith.

25 My own proposals are those which we reached two years ago, namely that crocidolite should be...the use of crocidolite should be prohibited except where it's absolutely necessary, where the substance has already been used and has to be handled, but where it has to be handled then we should insist on a standard of point two fibers per milliliter. For amosite, it should be half a fiber per milliliter, and for chrysotile, one fiber.

30 Five: Opinions relating to costs, benefits, etc., in respect of control limits, and in respect of substitutes, should be subject to the same expert scrutiny and criticism as is the scientific evidence relating to health effects in man and animal work.

5 THE WITNESS: (cont'd.) Six: In coming to these
conclusions I have given particular weight to the following points:
First, the indestructibility of the substances under discussion.
Second, the animal evidence which shows that all fiber types
tested are carcinogenic, given certain specifications with regard
to configuration of the fiber - i.e. chrysotile is potentially
as carcinogenic as amphiboles given the appropriate configuration
of the fibers. Third, the unlikelihood, in my opinion, of getting
further reliable data about the effect of low doses in man,
10 particularly reliable data which takes account of historical
differences in the specification of fibers used for different
processes, which I believe will be extremely difficult to recover
at this stage. Fourthly, the likelihood that it will be easier
to control the dimensions of fibers in relation to manmade mineral
fibers, than asbestos. Fifthly, I've taken into account the
15 absence of a demonstrable threshold, except perhaps in respect of
asbestosis, in regard to the asbestos-related diseases in man.

DR. DUPRE: Dr. Acheson, would you mind just
repeating the fourth factor in which you gave particular weight
to...that's the one just before the absence of a threshold?

20 THE WITNESS: Yes. The fourth factor is my opinion
that it will be easier to control the dimensions of the fibers
which are used commercially in manmade mineral fibers than asbestos.

In other words, if it were determined that as a
matter of public policy fibers of between point one and two point
five microns in diameter, and between ten microns and eighty
25 microns in length, should not be used in any substance, it would
be easier, I believe, for industrial groups manufacturing fibers,
so-called manmade mineral fibers, to comply with such regulations
than the asbestos industry.

30 DR. DUPRE: Have the parties got a batting order?
Mr. McNamee.

CROSS-EXAMINATION BY MR. McNAMEE

5 Q. Doctor, just going through one of those last points, and maybe this has come up already, so I'll just ask it anyway. Is there a government standards group, say in the U.K., for testing new materials coming onto the marketplace, setting standards in, say, for substitutes for asbestos?

A. Honestly, I can't answer that question. I do not know.

10 Q. Do you have any recommendations, or have you thought of having any recommendations that, say, making the introduction of new materials, the testing thereof, a joint government/industry responsibility? Have you had any ideas along that line?

15 A. I know that there is a working party advising the Health and Safety Commission on manmade mineral fibers, but I know nothing about its recommendations, I'm afraid.

20 Q. I noticed on one of your papers, I think it's study number six, you talk about record linkage and you express your hopes that maybe in the interest of better research that there could be some surrender of confidentiality of medical records. Is that your viewpoint, that some compromise should be made?

25 A. Yes. I think that it is possible for medical records to be used in large epidemiological studies, if you like, and for data to be assembled about exposure and illness in large files of people, without any threat to the privacy of the doctor/patient relationship. I think it's a matter of formulating the appropriate code of practice under the appropriate legislative authority, with special care taken that the public will be involved in any scrutiny of such studies. I think this can be done, and indeed Canada, perhaps more than any other country, has opportunities
30 to do this sort of work.

Q. Is there any draft model legislation on the books with respect to this particular point?

5 A. Yes. A number of countries in Europe have legislated, and the degree of success has been extremely variable. In some countries the effect of the legislation has been to prohibit any scientific studies of this sort, and such countries, I think, are trying to now change the legislation. But in Britain there has now been agreement that we will draft appropriate legislation within the next year or two to conform with EEC policy.
10 So far as I know, the draft is not yet published.

Q. Do you know whether this medical information could be coded in such a way that, say, if it was all placed in computers in a useful fashion that it couldn't be...that somebody knowing how it's coded could not get back to the name of the
15 individuals forming the basis for it?

A. I think it's possible to take care of this problem.

Q. Has that been discussed?

A. Oh, yes.

Q. Now, I know you discussed schools briefly and is there any actual program in the U.K. of inspection and
20 subsequent removal or monitoring of asbestos in schools and public buildings?

A. I'm not sure. I think I have a recollection that the local authorities who are responsible for the education program have been made aware of the possible risks of asbestos in schools and have been invited to check their schools. I'm
25 pretty sure this happened a couple of years ago when the Simpson Report was published.

Q. I think we had indication from one or two other witnesses...I gather you are aware of these studies by Nicholson and Anderson on schools in New Jersey, and in other places...some
30 of the materials, the asbestos materials, were probably put in in the late forties or early fifties. Would that be the same

Q. (cont'd.) experience in the U.K.?

A. Yes.

Q. So if there was any propensity to cause mesothelioma, at least for some people, would have had exposures in the neighborhood of thirty, thirty-five, forty years. Is that correct?

A. That's correct.

Q. You have indicated today in response to a question by Dr. Uffen, that you didn't think that, say taking a cohort of schools where you had asbestos, that a study...you probably couldn't get useful information. Is that more or less the...?

A. That is correct. I think it would be very difficult to identify the cohort.

Q. So going from that direction, it doesn't seem to be too practical. What about...I notice that in one of your studies you indicated that in 1975...I think it said in the Simpson Report...there were two hundred and fifty-six mesothelioma deaths in the U.K....knowing those, if you analyzed those two hundred and fifty-six carefully, going back from day one, do you think you could get any useful information bringing the schools into play, or would you run into the same roadblock? And you have a defined two hundred and fifty-six people that are dead, and you've got at least their records. It would be expensive, but at least you've got, say, only in one year you could track them all back and see whether you can somehow make a casual...

A. Yes, it would be possible to look at, say the last five years' deaths...give you fifteen hundred deaths due to mesothelioma, shall we say in rough terms, and controls, and compare the places where they were at school.

Q. You may or may not get meaningful data?

A. Exactly.

MR. McNAMEE: Those are my questions. Thank you very much.

DR. DUPRE: Thank you, Mr. McNamee.
Mr. Hardy.

CROSS-EXAMINATION BY MR. HARDY

5 Q. Dr. Acheson, I would like to just clarify a few things with respect to the quantitative risk assessment done for lung cancer, which is in table thirty-five of your report. As I understand your testimony earlier today, in beginning this risk assessment you reviewed the literature for those epidemiology studies with exposure information?

10 A. That is correct.

Q. At the time you did this report and this table, there were three such studies?

A. That is right.

15 Q. The McDonald study in Quebec, the Enterline study of production and maintenance workers in the United States and Canada, and the Rochdale study of the textile plant. I believe you have indicated today that if you were to update the study based on more recent information, there are at least two additional studies that would be included in this table?

20 A. That is correct.

Q. Those are the Newhouse and Berry study of the friction plant in the United Kingdom, and the Dement study of the textile plant in the United States?

25 A. That is correct, provided the Dement study actually has sufficient data on dose. I have only seen the preliminary report of it.

Q. Is it your view that the preliminary report does not contain sufficient dose data to be included in this study, in this table?

30 A. I'll just have to look at it again. I only saw it yesterday for the first time.

It doesn't seem to be here. I'm sorry.

Oh, here it is.

A. (cont'd.) Yes, it's got some data about dose.

Q. But I gather you haven't had the chance to review that dose data in very...very extensively?

A. That is correct.

DR. DUPRE: Excuse me, Mr. Hardy. Would you just permit an interjection here?

MR. HARDY: I would be glad to have it, Mr. Chairman.

DR. DUPRE: It is simply that my little notes indicated that perhaps in addition to Dement and to Newhouse/Berry, you would think, subject to...

THE WITNESS: Hans Weill.

DR. DUPRE: ...looking at the quantitative material, including two others as well - Hans Weill and Alison McDonald, of the same cohort as Dement?

THE WITNESS: Hans Weill. That is correct. Alison McDonald has studied the same cohort, but has not published any data about dust yet.

DR. DUPRE: Has not?

THE WITNESS: But is hoping to very soon.

DR. DUPRE: So in other words, the possible candidates for addition to table thirty-five then, would be three, and possibly four, studies?

THE WITNESS: That's right.

MR. HARDY: Q. And one of those studies, Dr. McDonald's study, is actually three different plants, so there might be three data points from her work?

THE WITNESS: A. One of them is the same plant as Dement. The interesting point is that she, in her study, says that there are two thousand, five hundred men in the plant, and Dement deals with seven hundred and forty-eight. I'm always worried about any cohort study which refers to a selected subsample of a cohort as a whole, and I feel sure that it will be a year or so...

A. (cont'd.) perhaps two years...before it's clearly possible to reconcile the different results of the two groups studying the same factory.

Q. I'm sure that's true.

One point on the Dement study, and you may not be able to answer it since you haven't seen it for very long, but as I understand that study, it involves the same issue of conversion from particle counts to fibers as was faced in the Quebec and Enterline study?

A. That must be the case if they are dealing, as I'm sure they are, with data collected before 1960. They have to be concerned with the conversion.

Q. Therefore we may have the same issue of differing conversion factors necessary to give the full range of the possible data?

A. This is true, and I note if you look at table thirty-two on page seventy-four of volume two of the Simpson Report, you will see that Dr. Dement has already used a very extreme conversion factor, relative to others, in relation to another matter. He used a conversion factor of four hundred, whereas Wagg and Bruckman used twenty, Nicholson, fifty-two, and Lynch and Ayer, five.

So I would be particularly interested to see what conversion factor he used in relation to fibers to particles. Here he is talking about fibers to nanograms.

Q. So that the conversion factor used by Dement you say is going to be an important part...

A. Indeed.

Q. ...of determining where in this table that study falls?

A. That is certainly the case.

Q. In constructing this table, as I understand it,

Q. (cont'd.) the standard mortality ratios in each of the studies was graphed against cumulative dosage, with a varied number of data points available for each study?

A. That is correct.

Q. There are other cohort studies in the literature, by Dr. Selikoff and others, which I gather you didn't use for this table, and the reason for that was that they do not report exposure information?

A. That is correct. They have no quantitative data about dust.

Q. It would be possible though, conceivably, could it not be that you could make an estimate of exposures for various workers in such studies? Would that be a useful addition to this table, or did you reject that possibility?

A. I don't think there's any way in which a third party not privy to the conditions in the factory and not being able to speak either to the employees or to the management, could usefully form any estimate. It's difficult enough if you are able to have access to all the data to find what is an appropriate estimate relating to practices that took place ten, twenty, thirty years ago.

However, what we did do in the appropriate part of the report...and it's page thirty-nine, paragraph 197...was to consider this as semiquantitative data. Paragraphs 197 to 200 are the best we felt we could do in relation to the Selikoff data and Newhouse work on Western Australian miners and so on, in coming to some conclusion about dose-response relationships and so on, where there wasn't any dose data.

But while it was useful background, we couldn't actually use it in advising the Committee to choose between options about a standard.

Q. Once you had determined the health evidence in these cohorts, and correlated the cumulative exposure, I gather

Q. (cont'd.) the result was a line which is the dose-response relationship in each of the three studies?

A. Right.

Q. I believe if we look at appendix six on page sixty-two, in the upper righthand corner, there are three equations that express the line for each of the three studies?

A. That is correct.

Q. This line, you then assumed, could be extrapolated down to a zero dose level?

A. That is correct.

Q. As I understand, one of your rationales for doing this sort of linear extrapolation is that the Committee believed that this would give a conservative, and thus more likely to be over than underestimates of relationship of disease to dose at the lower levels?

A. Yes. I think that is...that would be a fair representation of the view of the Committee, that on the whole a linear relationship would be conservative.

Q. I think perhaps if we look on page fourteen of your report, that's made explicit.

A. Yes.

Q. In paragraph forty-two.

A. Yes.

Q. Having determined these lines which express relationship between dose and response to determine the amount of excess disease, which you do in table thirty-five, I gather that you considered the relationship between the standard mortality ratio in any given dose, and the background incidence of lung cancer in the United Kingdom?

A. Not necessarily. It would depend upon...not the United Kingdom in relation to Quebec and Enterline.

Q. Let's back up a second. Certainly when Dr. Enterline did his study, he compared observed in his cohort...

A. Absolutely.

Q. ...to the expected, based on, I believe, national population rates in the United States?

A. That's right.

Q. From his work, you got various points on the curve and thus extrapolated the dose-response line?

A. Mmm-hmm.

Q. But then in constructing table thirty-five, don't you take the standard mortality ratio...excuse me.

In constructing table thirty-five, don't you take the dose-response ratio from Dr. Enterline's work and come out with that...from a conclusion for any given dose that there will be a certain percentage increase in lung cancer?

A. Right.

Q. Don't you then calculate to get the numbers in table thirty-five what that percent increase in cancer means for a general population, which in this case is the British population?

A. That is correct. What we are doing is trying to make a prediction as to what would happen to a British work force, but the numbers, of course, are, the numbers in the table, are dust concentrations in fiber per c.c. It's only the column heads that relate to excess mortality.

Q. Right. I believe that the background rate of lung cancer in the United Kingdom that you used is indicated in appendix X, is that ten percent of deaths in men aged fifty and above are due to lung cancer?

A. That's right.

Q. If the background rate of lung cancer in another nation, such as the United States or Canada, or in another era - say ten or twenty years from now - were lower than ten percent, would that affect the presentation of data in table thirty-five in that a one percent increase in lung cancer would mean fewer absolute

Q. (cont'd.) deaths because the background rate would be lower?

5 A. Yes, I think it is correct that countries with more favorable smoking habits than the U.K. will suffer less excess mortality from lung cancer related to asbestos, because of the synergistic effect of asbestos and tobacco. I think that is true.

10 Q. So I gather it's your belief that primarily responsible for the ten percent lung cancer death rate in the United Kingdom among men over fifty, is tobacco habits?

A. Sorry?

15 Q. I gather you believe that the factor primarily responsible for the ten percent lung cancer rate among men over fifty, in the United Kingdom, is smoking?

A. The most important factor, etiological factor, in Britain in relation to lung cancer is certainly tobacco. That is certainly true.

20 Q. In constructing table thirty-five then, I gather that one correction you didn't make, which could conceivably be made, would be to take into account smoking habits and determine what the excess risk would be at various fiber levels for nonsmokers?

25 A. I'm not sure that it would be possible to do that, because the data on nonsmokers is so sparse that it would be very difficult to form an estimate of asbestos-related cancer in nonsmokers...even if that were relevant. Most of the workers are smokers.

30 Q. Most of the workers today or most of the workers in the historical cohorts?

A. Both, in the United Kingdom.

Q. Do I gather from that that smoking habits among blue workers have not changed considerably in the United Kingdom?

A. Amongst who? Blue collar workers?

Q. Blue collar - the sort of workers we are talking about here?

A. They have certainly changed much less than amongst the middle classes.

5 Q. I guess the converse of that observation is that if smoking habits could be changed among those groups, they could have a substantial impact on the sort of risk we are talking about?

A. I have no doubt that that is the case.

10 Q. Having computed table thirty-five in your study, I believe you go through a description of what it means in context with some other occupational risks, and particularly you talk about the risk of radiation?

A. Right.

15 Q. Maybe it might be useful just to go through that discussion...

A. That's paragraph 259?

20 Q. ...and understand clearly what this group of numbers in table thirty-five means. Maybe we should look at where it is.

A. Paragraph 259?

25 Q. Right.

30 Or perhaps...there's one place...let me find the page...where you do it in more detail than that.

Perhaps you could just begin to explain the comparison to the radiation workers, that you made, based on that table?

35 A. Yes. Well, in the case of ionizing radiation the international health standard at the time this report was published - I believe it's the same, but I'm not certain - was five rems per annum, and according to the reference, one hundred and fifty, that was calculated on the basis of the fact that if a man was exposed to five rems per annum, each year for fifty years, twenty-two percent of the work force would die of cancer instead of
40 twenty percent. Not only would the two percent more die of cancer,

5 A. (cont'd.) but of course their lives would be shortened, because we all have to die anyway, so what you die of perhaps isn't totally relevant, but that means that not only do twenty-two percent die of cancer instead of twenty percent, but those two percent, their deaths are brought forward.

10 Now, according to which set of data one prefers to take, bearing in mind the conversion factors and so on and so forth, some point between zero point four fibers per c.c. and five fibers per c.c. will lead to a two percent excess mortality, of which one percent would be lung cancer and one percent to asbestosis.

15 Q. Just so that everybody can follow along, you get that by looking at the third column in table thirty-five?

A. That is correct.

20 Q. Which is headed by "one percent excess mortality from lung cancer" ...

A. That is correct.

25 Q. ...because you are assuming another one percent is caused by asbestosis?

A. That's correct.

30 Q. That's why we relate two percent?

A. Correct.

Q. And the point zero four is the ...

A. Zerpoint four is the level ...it's one of the levels of Enterline, if I'm reading across correctly.

35 No, I beg your pardon. It's the Rochdale level.

Q. It's the lowest level of the three...

A. Yes.

Q. ...for the Rochdale cohort?

A. That is right.

Q. The five is the...?

40 A. Is the top one of..

Q. From the Quebec data?

A. That is correct.

Q. The reason why you did this comparison to the radiation data was what?

5 A. To use the...to bring to the attention of the Committee the only other situation where an attempt had been made to reach a rational decision about public policy on the basis of epidemiological evidence, which involved a compromise between what is ideal, which is that there should be no excess mortality, and what is practical on the other hand.

10 So we used it as the only precedent, to put it that way.

One has to bear in mind that in the whole field of industrial carcinogenesis there are only two factors where it is possible to refer to any quantitative data whatever. One is radiation, and the other is asbestos.

15 If we feel that the asbestos data is indifferent, you should just look at the data for chemical carcinogens, of which there are no human dose-response relationships whatsoever.

So that is an attempt to put it in context.

20 Q. I gather from what you are saying then also that as uncertain as the asbestos human dose-response data is, it may nonetheless represent a much better data base than we have for making decisions for numerous other chemicals?

A. That is true.

25 Q. I gather it was the view of the Committee, and your personal view, that it would be desirable to make the best possible use of this data base in trying to come up with a rational decision?

A. Correct.

30 Q. Moving from table thirty-five to table thirty-six, I gather that what the Committee did was to further extend the extrapolation of dose-response relationships from occupational levels down to levels that might be found in various settings in the general population?

A. Correct.

Q. But it's the same underlying data...

A. Exactly.

5 Q. ...but much lower dose levels that we are looking at now?

A. Exactly. And one is taking as examples the highest level ever measured, detected, in a British city out of doors - that's the lefthand column - ten nanograms per cubic meter, converted to fibers per c.c., and on the righthand we are taking
10 the median and the highest level recorded in a British building by the only survey we had to hand, namely the Byrom survey in buildings.

15 Q. I gather from what you said this morning that the conclusion you drew with respect to general ambient concentrations of asbestos was that even with this, was that this data gave you a great deal of confidence that there was no significant risk at all to the general population from such exposures?

A. That is correct, with the exceptions I mentioned about asbestos dumps uncovered and certain specific problems in
20 buildings.

MR. HARDY: Dr. Uffen?

DR. UFFEN: Could I ask a particular question that falls in here, and puzzles me?

Earlier on when we talked about reasons for preferring a linear hypothesis, you gave three. That would be...

25 THE WITNESS: Right at the beginning, I think.

DR. UFFEN: ...item forty-two.

The third one - we've heard this several times - it's likely to be to an overestimate rather than an underestimate of risks at very low doses.

30 Now, we are talking about very low doses right now. I have never been able to understand the reason for coming to that

5 DR. UFFEN: (cont'd.) conclusion. We're extrapolating data to very low doses. We argue night and day as to whether it's a straight line, curved this way, that way, conversion factors can be different by factors of up to six, and then we say, but it's likely to be underestimated. Why?

10 THE WITNESS: I think we are in a field in which there is a great deal of speculation and very little evidence, but I think that those who argue along these lines argue from two principles. One is the experience in certain exposures in animals, and the second is the most recent data relating to the dose-response effect in relation to cigarette smoking and lung cancer, where in the most recent work Richard Peto and Sir Richard Doll assume that, they are of the opinion that the relationship is not linear, but that the line is bent close to the origin, downwards. That is to say, concave upwards.

15 DR. UFFEN: That's for smoking and lung cancer...

THE WITNESS: A. Right.

DR. UFFEN: ...without any asbestos?

20 THE WITNESS: Absolutely. There is no scientific evidence from which, empirical evidence, from which you can argue directly, other than the evidence which we...in my opinion...other than the evidence that is before you in this report. The actual empirical evidence is based on an extrapolation in which it's a matter of opinion which way it bends, and you can say if the opinions are equally divided as to whether it bends downwards or upwards, a straight line is a fair compromise.

25 DR. UFFEN: If I were to come to the conclusion that a straight line was the best compromise for the first two reasons, and rejected the third reason, that would be equally ...?

30 THE WITNESS: I think you are totally entitled to come to that view, and it could be supported just as well as the view of the Committee, which was that this was likely to lead to an overestimate.

THE WITNESS: (cont'd.) I honestly think we are in a field where we are dealing with academic speculation.

DR. UFFEN: If you don't mind my sneaking in...

MR. HARDY: No, fine.

DR. UFFEN: ...it was low doses.

MR. HARDY: Fine.

MR. HARDY: Q. Dr. Acheson, I gather that in your extrapolation of the line to the evidence of exposure levels in buildings, you also concluded that with the exception of the rare building there was not a significant public health risk?

THE WITNESS: A. We did conclude that, but we also...we did underline the fact that we were dealing with very scanty data, and we made a recommendation that there should be further surveys in England. The Commission is the best judge of what ought to occur in Canada in that regard.

Q. Are you aware of whether that further survey is being done by the Health and Safety executive?

A. I am not aware. I believe that local authorities were encouraged to do their own local surveys, and they may very well have done them and I might not know about it.

Q. But I gather from the discussion earlier today that you were concerned that there may be buildings like the few you knew about when you wrote this report, where exposure levels were comparable to occupational exposure levels?

A. That is correct.

Q. It was certainly the opinion of the Committee that those sorts of exposure levels require some sort of corrective action?

A. Indeed. Indeed.

Q. But I gather that you also were of the opinion that there were no doubt buildings, school buildings, which may have had asbestos used in the construction which did not present exposure levels of that order of magnitude, and thus probably did

Q. (cont'd.) not require corrective action?

5 A. I don't think the Committee took any evidence on school buildings, other than the Byrom report, which if you note refers in table thirty-three to eighteen public buildings used for education - which presumably were likely to be schools - which happily all had very low levels.

Q. And levels which I gather...

A. Except for one. There's one which was a little bit...between zero point zero three and zero point zero four.

10 We know that these were recently-completed buildings, and they were not, in this survey, they had not had the opportunity to be used by pupils, and damaged. That is for sure.

15 Q. But I gather it was the view of your Committee that buildings with levels of exposure like the other eighteen, this is under Education in table thirty-three, meaning exposure levels below point zero one fibers per centimeter were probably not posing any significant risk to their occupants?

20 A. Certainly they would not be, as long as the level stayed like that. But if these buildings had asbestos spray on the walls or steelwork, pipes, etc., the Royal Institute of Chartered Surveyors felt that they were highly susceptible to damage during normal use, and I think the difficulty here is that these were, we know, buildings that were in process of completion or just completed. They might not have even been commissioned. So we simply don't know what would have happened or might happen in the future.

25 Q. Thus you are being cautious because monitoring results just were not available to your Committee..

A. Exactly.

Q. ...on schools that had been used for a number of years?

A. That is correct.

30 Q. Which may have had exposure numbers higher...

A. Might.

Q. ...than the ones found here in table thirty-three?

A. Might or might not.

5 Q. One other public health risk, or potential risk or nonrisk, which is discussed, but not extensively, in your report is the question of whether any risk is posed by asbestos in drinking water.

A. Yes.

Q. Was that issue considered by your Committee?

10 A. It was considered and our recommendation was, or our view was, that as far as the United Kingdom was concerned, we had not data. The only data we could find were one or two items of data from the United States and, I believe, perhaps one or two from Canada and one or two from Europe. So we felt that there was a need for further study.

15 But I think it would be fair to say that the Committee as a whole tended to discount a risk relating to drinking water.

Q. When you discussed the data that was available to the Committee, are you talking about health data or monitoring data, or both?

20 A. Principally monitoring...there was no information which made it possible to determine how many fibers there were in British drinking water, and the only study which was available at that time which related health effects to fibers in the drinking water was the study from Duluth, which hadn't been, which didn't show a very long latent period. But there is an editorial in this week's Lancet, which I hope I...in fact I know I've brought
25 with me...in the Lancet of the 18th of July, there is an editorial entitled Asbestos in Water, which refers to a study carried out in San Francisco which Conforti and other, C O N F O R T I and others, have published a paper in the Journal of Chronic Diseases, Volume Thirty-four, page two hundred and eleven, of this year, in
30 which they have demonstrated a statistical association between asbestos in drinking water and cancer in the San Francisco Bay area. I haven't seen the article. I happened to read the review

A. (cont'd.) on the plane, quite by accident, coming to Canada.

5 Q. But I gather at the time your Committee reviewed the evidence they found no human evidence of any ingestion risk?

A. That is correct.

Q. Which is, as I understand, what's stated on page fifteen of your report, is consistent with the absence of any evidence of risk in the animal ingestion studies?

10 A. That is correct.

It is dealt with in a little bit more detail in paragraph two hundred and forty, on page forty-six.

Q. Right. Where you talk there about the Duluth study, where no excess of cancer deaths was found, and the study in the Netherlands.

15 A. That's right. However, we do point out the inadequacy of epidemiological methods in picking up small increases in relative risk in very large populations. It is...it has to be taken into account that if exposure is massive in terms of perhaps two-thirds of the public water supply in the United Kingdom, at a small rate, it would be possible to miss a risk which caused some hundreds, or possibly a few thousands, of deaths. I think this is the problem when one deals with massive...very large populations exposed to low doses of a substance.

20 So I just have that little proviso to put to the Commission, that epidemiology is not a very sensitive tool in that sort of situation.

25 Q. It's in cases like that often, though, isn't it, that scientists look to the animal evidence where higher doses are able to be used in order to test carcinogenicity?

A. Yes, I think that's a fair comment.

30 The animal work is summarized in paragraph 241, on page forty-seven.

5 A. (cont'd.) I must say that at the time that this report was published, there did not seem to me to have been a sufficient...I'm speaking personally now...sufficient number of experiments in this field. It may be that others have been published since.

Q. Are you aware of the studies being done by the U.S. government...

A. I've heard about them.

10 Q. ...on feeding asbestosis?

A. Yes, that's why I was a little bit cautious about what I said there, because I'm not aware whether they have been published.

15 Q. I don't believe they have been published yet, although they are coming close to completion and some of the results have been announced.

Needless to say, to sum up though with respect to ingestion, I gather your Committee did not find it significant enough a problem that it felt should be addressed with respect to ingestion of asbestos by the general population?

20 A. No data in the United Kingdom, and I think the Committee...I think it would be fair to say the Committee did not feel it was top priority in respect to further studies on large scale, from a limited budget, but other studies relating to buildings and various studies in relation to industrial exposure were more important.

25 MR. HARDY: I have no further questions, Mr. Chairman.

DR. DUPRE: Thank you, Mr. Hardy.

Who wishes to go next?

Miss Jolley, please?

30 CROSS-EXAMINATION BY MISS JOLLEY

Q. My first question is dealing with mesothelioma.

Q. (cont'd.) It's our sense in your testimony and in the testimony of Mr. Berry that there is a great increase in mesothelioma happening, in U.K. especially. Is that correct?

A. There is a graph in the report which shows the...figure seventeen... Assuming that the population has remained the same, which is roughly correct, it's fair to accept that trend. I think you also have to take the countereffect that diagnosis has improved, but I think everyone would agree that in Britain the number of cases of mesothelioma, and the actual incidence rate of mesothelioma, is increasing.

DR. DUPRE: Figure seventeen is what page?

THE WITNESS: Eighty-three.

I think one should also say that we expect that it will continue to increase, and it would increase if asbestos ceased to be used and nobody was exposed to it from tomorrow, until at least the end of the century. So that some substantial part of this increase represents conditions of work that are no longer applied, and indeed a lot of it represents exposure to crocidolite which has now virtually ceased in the United Kingdom.

Q. Is it fair to say that smoking has no impact on mesothelioma?

A. That is correct.

Q. So that by altering the smoking habits of the working population in the U.K. or in North America, you wouldn't necessarily alter the incidence of mesothelioma, if the levels remained the same?

A. I would go further than that and say you would not influence it, even without the word 'necessarily'.

Q. Okay. Then I would like to move on to fiber and on page thirty-four, you dealt with the whole issue that you dealt with on two occasions today as well, and that's the issue of fiber specification for chrysotile. You indicate on thirty-four that there is some evidence that you were not able to substantiate

Q. (cont'd.) from Quebec?

A. That's right.

Q. That perhaps...

5 A. I've been looking for that footnote. That's the footnote I was referring to. Page thirty-four, the lefthand column.

10 Q. Now, it's my understanding that your recommendation for a one fiber level for chrysotile is based on the fact that we would continue to use, generally, perhaps longer fibers, and you are dealing with what the risks are now, and therefore you are dealing with...you are not dealing with the movement to the shorter, finer fibers? Is that correct?

15 A. Yes. In looking at table thirty-five, one is looking at extrapolations from health effects which are the result of the use of asbestos twenty, thirty years ago. That is correct.

20 So that if it is true that they chrysotile being used today is more dangerous than the chrysotile twenty years ago, that is not taken account of. It's also true to say that if it's less dangerous, that's not taken account of either.

25 Q. If, for instance, there is a major move to the shorter fibers, the short, fine fibers, would you recommend a more stringent standard very much like crocidolite, the crocidolite standard?

30 A. If the trend of exposure is that most people in future are going to be exposed to fibers that are closer to the configuration that causes cancer in animals, well then, I think that has to be taken into account in setting the standards. But I think you have to take into account what are the trends within the industry.

If you look at table two A, you can see that when we studied the matter in 1976, about forty percent of the industry was asbestos cement for buildings and pipes, and we have been

A. (cont'd.) advised that the trend there is in the other direction - to less, quotes, 'dangerous' fibers.

5 So I think one would have to take into account how these two trends were working, and this was...this is a vital area of evidence, in my opinion, for your Commission.

Q. So there is the possibility that not only might you set separate standards for differing fibers, but you might set separate standards, in fact, even for chrysotile fibers, for different usages of chrysotile?

10 A. Yes, that was a possibility that we considered, and we were advised by the people who are responsible for enforcing standards generally - they said, make it simple, please. The more difficult you make it, or the more complicated you make it, the less likely we are going to be able to enforce it, basically.

15 Q. Speaking of making the measurements simple, too, is that when you deal with a phase contrast microscope, and we've had a fair amount of testimony in addition to your own, that the phase contrast microscope is not necessarily measuring the biologically active fibers, and there is a reference in your...it's of some concern to us that you are not measuring what in fact you are
20 worried about.

Had you looked at the possibility of electron microscope as the measuring tool?

25 A. Yes. It was discussed and it was felt that it was impracticable in terms of cost, at the moment. That the machine is so expensive still, and the ability to use it is so rare, the skill of being able to use it properly is so rare that it wouldn't be practical politics to ask industry to use this generally...yet. Perhaps it will come in later.

30 Q. Again, on measurement, you made a comment to us earlier about Dement's dust levels, and that, you know, you would like to see further evidence about his dust levels to see if they approximate the truth.

Q. (cont'd.) There was a quote that you had on page forty-nine, and I wondered if you could just comment on it. It's in the lefthand column of page forty-nine.

5 You are discussing the conversion factors, and I think that it's important for us to understand but you've said, "As has been pointed out, this at least introduces additional uncertainties, and at worst is indefensible."

10 I think you are discussing the whole conversion factors as presented by Gibbs and LaChance, and that's the conversion factors used in the McDonald study. I wonder if you could further comment on that in terms of our assessing your risk factors?

15 A. Yes, that's right. Some people would say that to try and transform data from one unit to another, where the correlation between the methods is poor, is indefensible.

One gets back to the same point that you then have to decide whether to do it or not to do it. The Quebec people have disclosed the great difficulties that they had in making correlations, but many of the others haven't disclosed them at all.

20 There is no information in the Dement paper, there is no information in Rochdale, and I'm pretty sure there's no information from Enterline. One can reasonably assume that the difficulties and the lack of correlation was just as great in all of them. I can't see why it should not be.

25 So I don't think that it's right to say that Quebec is necessarily worse than the others. I think they are all difficult and you have to make the decision whether to use them or try and arrive at the standard on purely pragmatic grounds without taking into account any attempt to use the dose-response relationships.

30 Q. But it does put the whole issue of setting standards just based on that evidence, into some suspect?

5 A. Oh, indeed. I think that would be true of all other standards, particularly those that don't use the data at all, which is true of polycyclic hydrocarbons, aromatic amines, and so on.

10 Q. On that same page you said, you were talking about the linear hypothesis, and you said the linear hypothesis may overestimate the risk, but the Quebec data and the Enterline data may underestimate the average risk to men exposed, and I think it has been indicated to us that the Enterline data is based on a survivor population, and therefore may very well underestimate.

Could you tell us why you think the Quebec data underestimates risk?

15 A. It might underestimate it because of the nature of the fiber at that point, that the fiber to which miners and millers are exposed is all the fiber that comes out of the mine, in the state that it comes out of the mine. Whereas, if you work in a textile factory, you are exposed to a particular sort of fiber that has been milled and prepared for a particular purpose. So therefore, it may be...they may be at a greater risk than the people in the mines.

20 It depends also on how much the fiber is opened at the mine. The tendency has been, we were advised, that over the last twenty years the fiber has been opened to a less extent at the mine, and to a greater extent in the actual factory where it's used than was previously the case.

25 Q. That certainly has been substantiated in other evidence before our Commission as well.

A. Yes.

30 Q. But one would not, therefore, use a mining population study to determine the risk for a textile mill, for instance?

A. Well, the question is, how else are you going to do it? The great strengths of the Quebec data are that you

A. (cont'd.) have far more cases - two hundred and fifty deaths from lung cancer - than in any other study, and also you have a number of points at low levels.

5 If you look at the dose-response relationship, there are quite a lot of points near the origin, which when you are dealing with current practice where inevitably the levels are much less dusty than they were fifteen, twenty years ago, this is particularly helpful.

10 In contrast, the number of lung cancer cases used both by Rochdale and Dement is very small. In Dement's cases here we have twenty-six cases of lung cancer, and in the Peto case, it was, I think, about the same or possibly a smaller number.

15 Q. I would like to move along now about...you mentioned two provisos about public exposures, and one of them was well, first of all the public buildings issue. I would like to talk about it, because you talked a little bit about childhood mesothelioma, and I found that that testimony was extremely interesting. It's something that had caught my eye when I read through.

20 But it strikes me that another proviso might be, and I wondered if you concurred, that if in fact the exposures are where young children would be, if there are...in a public building sense...if the potential exposure is to young children, would that also be of concern then?

25 A. I think so, yes. I think that in any extrapolation from an industrial population to the public as a whole, one has to take account of a number of points, and I believe that these are usually taken into account by toxicologists. First, that the risks to children, elderly people, pregnant women and so on, may be different to the risk of a healthy work force, or a work force that's healthy to begin with, anyway.

30 Secondly, that the time of exposure may be different, and in the case of the home, may be up to a factor of

A. (cont'd.) three longer than the eight hour day. People may be at home for twenty-four hours a day.

5 So that all these points have to be taken account of, and it's prudent practice to give an additional safety factor in relation to children and elderly people.

10 The other point that one has to bear in mind is that a child, whatever may be the truth about his susceptibility, has seventy years to live and to suffer the result of exposure to something in childhood, whereas an adult in the work force, on average, may have thirty-five or forty years to live.

15 Q. The other thing that you talked about in your discussion this morning about childhood mesothelioma was the obvious variability then in individual latencies, that there is a possibility that there could be in fact quite short latencies as opposed to the statistical measuring of latency in individual cases. I wonder, therefore, in terms of compensation for cancers related to asbestos, would you also consider that fairly rigid guidelines as far as latencies are then not appropriate, that they should be judged according to individual cases?

20 A. Well, I think...I'm not speaking personally, because at no stage did we address the issue of compensation in the Simpson Report...I think it depends which cancer you are talking about. If you are talking about mesothelioma, I think that the conditions under which it's reasonable to consider a case to be attributable to asbestos are different and should be much wider than in the case of carcinoma of the bronchus where one has to take account also of the fact that there will be a

25 substantial of cases quite independent of exposure to asbestos, due to the tobacco problem. So that I think I would be personally prepared to accept a very much wider period of latency in respect of mesothelioma, and also a period of exposure, than in lung cancer.

30 Q. My last question is concerning, again, your paper on record linkages and confidentiality, and I think as

5 Q. (cont'd.) members of the group to be studied are people are concerned about confidentiality, and I think that our concerns could be eased a little bit if we were part of the study preparation, etc., and consulted with, of course.

But one of your comments on page a hundred and seventy-six was of a bit of concern to me.

A. A hundred and seventy-six of what?

Q. I'm sorry. Tab six, on record linkages.

10 You are discussing the problem of confidentiality and whether or not informed consent of a patient be necessary for medical records, and a concluding sentence of that paragraph says: "It would, for example, be entirely inappropriate to seek informed consent from workers to use their clinical records where the strength of the a priori evidence of the existence of a carcinogen did not justify raising their fears".

15 I think that it concerns us that there is an attitude among researchers and among physicians that we will be overly concerned about cancer, and it has been our experience in the past that we have a need to be concerned about cancer in the work force, and I think certainly even in the future workers would consider that to be true, too.

20 A. Yes.

Q. I just would like you to comment on that?

25 A. Yes, if I could respond to that, I would accept that that sentence could be interpreted as being paternalistic and arrogant...if it isn't taken in context of the view which I hold that the decision as to whether or not a group of people should be informed should not be taken by the research worker on his own or her own. It should be...on the other hand, I do believe that there are circumstances where it is inappropriate and totally selfdefeating the have to get the consent of every single person in a work force, including those

5 A. (cont'd.) people who have left. None of the cohort studies that we have been discussing could have been carried out if it were regarded as essential to...before you could, Dr. Dement could study his group...that all the persons who had left and the relatives of all those who had died have to give consent before they could be followed. You see, that's the other extreme.

10 My view is that there has to be some sort of committee which acts as a surrogate, including representatives of the work force and an independent person who is neither a member of the work force nor the management...and of course, members of management...to come to some view as to what stage it is appropriate to explain to people what is happening.

MISS JOLLEY: I have no further questions. Thank you very much.

15 DR. DUPRE: Thank you, Miss Jolley.

M. Bazin?

CROSS-EXAMINATION BY M. BAZIN

20 Q. Dr. Acheson, I understand that there is not, or there was not at the time of the writing of the report, any mining or milling of asbestos in the United Kingdom. Is that correct?

A. No mining, and to the best of my knowledge there never has been, but milling in the sense that now a number of process firms open the fiber at the beginning of the work, doing work which used to be done at the mine. That's done.

Q. So that is being done?

25 A. Oh, yes. For example, fiber is opened further in preparation for work to make insulation boards, at the plant, which I think used to be done at the mine. But in the sense that you mean is there any operation of mining and milling like in Quebec, of course there isn't.

30 Q. In that connection referring to the standard, and in that context of mining and milling as we both understand it now, would you agree that the McDonald survey would be the best

Q. (cont'd.) available on the mining and milling employees?

A. In relation to chrysotile, yes.

Q. In relation to chrysotile. Yes?

A. There is no other quantitative data that I know of, either from Rhodesia or Cyprus, or the Soviet Union.

Q. Is there any, in your opinion, evidence in that survey that would lead you to conclude that there should be a change in the standard, say from five to two, or from two to one?

A. I couldn't answer that question because a decision about what is an appropriate standard, in the Province of Quebec, is not just a scientific question. It's a question relating to all manner of things which I am totally unable to comment on.

Q. I therefore assume that when you state your opinion as to the one fiber standard for chrysotile, it was arrived at in the context...not in the context of mining and milling as we have it in the Province of Quebec, is that correct?

A. It was arrived at as a recommendation to be considered for the United Kingdom.

Q. If I understood correctly your statement this morning, it was a unanimous consensus?

A. It was.

Q. Like you also mentioned, the one fiber chrysotile standard in the United Kingdom, industry felt that it could adhere to that standard, is that correct?

A. I'm sorry. Could you say that again?

Q. In the Committee, industry representatives felt that they could meet that one fiber per c.c. standard?

A. Yes, I think they did.

Q. You even said that without difficulty, in the United Kingdom always. Is that correct?

A. Yes, that's correct.

5

Q. Just to clarify a point on the question of the counts of fibers that were carried out in the United Kingdom, and which you alluded to and which is page seventeen of volume one, figure one...

A. Yes.

10

Q. Do I understand correctly, for instance, if you look at the industry section you would have eight hundred and forty-five results?

A. Mmm-hmm.

Q. And you would have a max of six point two fibers, is that correct?

A. That is right.

15

Q. Okay. On the other hand, you would have ninety-eight point five counts below two?

A. That's right.

Q. It goes as high as thirteen point six six, I guess in insulation board?

20

A. Yes. I think it's quite noticeable that manufacturing of insulation board is associated with the least satisfactory distribution on that table.

Q. That is from November, 1972, to February, 1978, I guess, that these were looked at?

A. That's right.

25

Q. Can I refer you, just for clarification, to volume two, now, in connection with standards - page ninety-five. Table one, Canada, eight hours, five - Quebec. That is the five maximum fiber...I'm not sure if it's..it's number of fibers per ML equivalent. That's the five maximum? Two is the number of fibers?

30

A. So the hygiene...this is incorrect, is it? The hygiene standard for Quebec is not five?

Q. It's two, and five number to be exceeded.

A. I see. Well, please accept my apologies.

Q. It would therefore, if I just look at the table that you have, coming back to volume one and assuming that there are no peaks above five and the standard is followed, that it would be more stringent than the U.K. standard between 1968 and now?

A. That I don't know. If your standard is two fibers in Quebec, it is the same as the standard in the United Kingdom...

Q. Maximum five. Never to exceed five.

A. I don't...I think we just have the one standard, which is that you shouldn't exceed two, in the United Kingdom.

But of course there is nothing in the table that you have referred to, about mining...that you pointed out.

Q. That's right.

A. Yes.

Q. Again, in your proposal about the one fiber for chrysotile, that would be not to exceed one fiber of chrysotile?

A. That's right.

Q. In the context not of milling and mining as we understand it in the Province of Quebec.

In the same connection about tailings, you made passing references to...dumps, I should say, and tailings...are you aware how the mines in Quebec dispose of the tailings, or how these tailings are kept?

A. I have been to Thetford.

Q. You have?

A. I have.

Q. So you have seen the situation there?

A. I have.

Q. Are you aware that there have been some surveys carried out about the local population in Thetford Mines and Asbestos, compared with other cities in the Province of Quebec, which would indicate no increased risk of cancer for the female

Q. (cont'd.) population?

A. No. I know of two surveys of mesothelioma in Quebec, but I don't know the study you refer to.

Q. I would like to ask you a few questions on the matter of cofactors, that you very briefly referred to in the report on page thirty, at number 146, in the context of the various types of fibers.

A. Yes. This is about mesothelioma?

Q. Yes, in that context.

There, in the three options, you reject the possibility of the interference or action of cofactors.

A. Yes. For the reasons stated, we took the view that the most likely explanation for the mesotheliomas occurring in American insulators, who are not exposed to crocidolite, was exposure to amosite, and I believe that that view has been strengthened by subsequent data which I referred to this morning, namely Dr. Alison McDonald's study of the lungs of North Americans with mesothelioma as compared with controls, which show an association with amosite as well as with crocidolite.

Q. So you hold the same view insofar as the participation and/or action of other substances in the cases of lung cancers?

A. I'm sorry. I don't follow you.

Q. I'll rephrase that.

A. Yes.

Q. Paragraph 146 is in the context of mesotheliomas?

A. Right.

Q. If I got back to lung cancers now, did the Committee consider other cofactors than tobacco as a possible initiator, participant or...?

A. No. We considered tobacco, but nothing else.

Q. Nothing else?

A. No.

Q. On tobacco, I believe in one of your documents which is tab eight, at page 748, under paragraph Other Factors, you refer...there is a sentence, and I quote, which reads:

"Both these factors should be taken into account in formulating public policy".

A. I'm just trying to find it.

Q. Page 748.

A. I've got page 748.

Q. Just at the bottom.

Mr. Laskin asked you questions about unknown factors.

A. Yes.

Q. At the sentence just before that one.

A. That's right, and we are referring to smoking cigarettes in relation to the risk of lung cancer in asbestos workers... "The importance of geological types of fibers in relationship to the risk of mesothelioma are best worked out from the epidemiological point of view. Both these factors should be taken into account in formulating public policy."

Q. In your expert opinion, could you elaborate as to how both these factors could be taken into account in formulating policy, or in helping this Commission in making recommendations leading to the establishment of policy?

A. Well, with regard to fiber type, it is my view that...as I have said already...that as an interim measure... that is to say, at present...it is in the public interest to apply a differential to the control limit for the three commonly used types of asbestos, and I believe that the Simpson Committee's recommendations, which I needn't repeat, are practicable and sensible.

With regard to the tobacco question, this statement relates to the public policy not in respect of control limits, but in health education, etc., in relation to workers who smoke, continue to smoke, and are exposed to asbestos. I feel that every

A. (cont'd.) measure should be taken to explain to them the risks of smoking in general, but in particular if they are exposed to asbestos.

5 But I do not consider...and again I'm expressing a personal view...that the tobacco factor in any way reduces the responsibility of the asbestos industry to control their standards, to control the dust levels. I think it should be no more dangerous... and I am again expressing a personal view...to smoke on the work floor as in the board room, in an asbestos factory.

10 Q. Why do you reject the nonsmoking policy as a control measure? Is it because it's impractical, or it couldn't be enforced, or why is it left to an education type of approach, in your opinion?

15 A. You mean to...I would have no objection to any measure which forbade smoking in the factory. This is common policy in many factories, all chemical factories, in furniture factories. One isn't allowed to smoke for various reasons.

20 I don't see any objection to that, but on ethical grounds, I'm not sure that I would agree that one could... well...on practical grounds you can't limit the work force to nonsmokers. You will get no workers. Put it that way.

DR. UFFEN: Well, if I may...am I correct that there is a distinction in some other places that people are not allowed to smoke if there is a hazard to others as a result of the smoke? Inflammable materials?

25 THE WITNESS: That's certainly true in coal mines, and it's true in the furniture factories. I'm not sure whether it's necessarily the case in all chemical factories, that there is an inflammable risk.

30 But I take the point and I accept that there is a distinction and that it's one thing to enforce a no-smoking in the interest of the fire risk and so on, and another in respect of a personal risk. But of course you have to take into account that

5 A. (cont'd.) in the last two years there have been compelling studies showing that there is indeed an important sidestream public health effect in relation to tobacco smoke, and that the wives of smokers in populations both in Japan and Greece, where women have not yet taken up smoking to any extent, are put at risk in terms of lung cancer by the smoking habits of their husbands. I would have thought it's quite likely, epidemiologically speaking, that smokers in asbestos factories, smoking where there is asbestos, are not only increasing their own risk, but increasing that of workers sitting beside them.

10 Q. My question is related to public policy insofar as it would be applicable to the workplace - for instance, in a mine or...it's in that context that I wanted to have your comments about a policy which would impose a smoking ban in the workplace.

15 A. I think that in the light of current information about tobacco, and the probability of a risk due to so-called sidestream smoking, that it would be quite proper to consider banning smoking during work hours in an asbestos factory.

M. BAZIN: I have no more questions.

20 DR. DUPRE: Thank you, M. Bazin.

Dr. Uffen, any questions?

DR. UFFEN: No.

DR. DUPRE: How about you, Dr. Mustard?

25 DR. MUSTARD: I guess I have a problem trying to relate biological facts and epidemiology. The problem is partly generated by some of your comments today, some earlier comments.

30 When asbestos fibers are inhaled into the lung, their handling would likely be different than if you ingested asbestos fibers through the esophagus and the gastrointestinal tract, and I wonder, therefore, if there is a reasonable probability that the asbestos fibers may be passed through the alimentary tract a little more rapidly than the respiratory tract, just simply penetrating the gastrointestinal wall.

DR. MUSTARD: (cont'd.) In your epidemiological studies and trying to look at gastrointestinal cancer, you may be bedeviled by the same thing, the problem of the population you've got to look at.

In other words, let me put it this way: You take asbestos fibers into your lung, they may hang around for quite a while, and therefore you'll get those cohorts and look at them, whereas people who take asbestos fibers and get them in the gastrointestinal tract, the period in which you will get the maximum impact of that may well be at the point where the workers are getting maximum exposure to asbestos fibers - not twenty years years later when they have been taken away out of the asbestos environment.

Do you know if there's any data suggesting that there is a time effect in terms of exposure to asbestos, and gastrointestinal cancer? Let's put it this way - that the bulk of the studies showing a positive effect are those studies in which the work force is being sampled while it's still being exposed to asbestos fibers, rather than people who have been taken out of the asbestos environment?

THE WITNESS: I think a number of the...the Dr. Selikoff, whose three different samples of insulators and insulation makers really provide more than sixty percent of all the evidence about alimentary tract cancer, would say that many of the alimentary tract cases appeared very late, and that early followups of his cohorts didn't show the full range of increased risk. The first risks that he saw were lung cancer, then mesothelioma and then alimentary tract.

So I think these alimentary tract effects occur two or three decades after first exposure. But that doesn't necessarily follow that the mechanism is...if there is...if the effect is truly related to asbestos and not to some cofactor, that the pathophysiology is anything like the pathophysiology in the lung. In fact, I think I'm right in saying that no one has

5 THE WITNESS: (cont'd.) satisfactorily demonstrated, either in animals or in man, or perhaps they couldn't...but in animals, that asbestos fibers actually penetrate the alimentary tract. I don't think that has been demonstrated.

10 DR. MUSTARD: I guess my question, really, is, if you have a situation in which in a site, for the carcinogenic effect to be shown, that need for continuous stimulation by the stimulus, in the case of gastrointestinal cancer if you take the worker out of the exposure to asbestos, is the way the gastrointestinal tract works, the biological probability is that you fairly rapidly clear the individual's exposure and therefore that the risk of gastrointestinal cancer might be best correlated with people who have continuous exposure to asbestos.

15 I wonder if in any of the epidemiological studies anyone has looked at that?

THE WITNESS: I don't think so, but I think one should say that people - exasbestos workers - cough up sputum with asbestos bodies, and asbestos bodies are amphibole fibers covered with ferritin and various other...

20 DR. MUSTARD: So they might keep seeding the gastrointestinal...

THE WITNESS: I would guess that most of that sputum is swallowed, generally.

25 DR. MUSTARD: One of the reasons for asking the question is that in the McDonald study of the Quebec mines, the data..and when he was here we asked him the question and he, himself, had commented that he had not taken that much notice of it, that there is a trend for the gastrointestinal cancer to show up in the earlier block of individuals, the short exposures etc., in the system, and which raises the series of questions in my mind about trying to relate the epidemiology to biological studies.

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5 THE WITNESS: I think one of the things about the McDonald study and the alimentary tract cancers was that there were a number of irregularities that made one wonder whether he had a genuine biological fit, because he demonstrated either in Asbestos and not Thetford, or Thetford and not Asbestos, one or the other, and there were certain other irregularities about it which made it rather unconvincing.

DR. MUSTARD: Thank you.

10 DR. DUPRE: Dr. Acheson, you have had a long day. If you could just indulge me in a couple of quick questions, I want to go back to page thirty, paragraph 146, that you were discussing with Mr. Bazin, where you look at the possible explanations for mesotheliomas, and there are...and on balance come down with the first of the three possible explanations, namely that they may be due to exposure to a mixture with a
15 biologically proportion of amosite.

20 Just so that I can understand that point in context, can I take it that the judgement that you are making there is based on material that is different from the Pooley set, because you see, you refer to the Pooley study in paragraph 141, on page twenty-nine, and as I read paragraph 141, the judgement that you reach there seems to be very, very similar to the one that we tentatively put forward in what is tab three in your exhibit?

THE WITNESS: Yes.

25 DR. DUPRE: A judgement which you then, as I understand it, very cleverly qualified in the letter that you wrote that appears in tab seven?

30 THE WITNESS: Right. Thank you, Chairman, for giving me the opportunity to tell you that I do not think that the observation that we made that there was a synerism between chrysotile and amphiboles in relation to mesothelioma has stood up. I think first of all when we looked at in relation to a second sample of controls provided by the same source, we found no

THE WITNESS: (cont'd.) relationship, and Dr. McDonald, Alison McDonald, in her study of this large sample of North American mesothelioma cases and controls, there is no evidence of a synergism between chrysotile and amphibole.

So I think that the safest thing to do with regard to that observation is to withdraw it, which is what we did with our letter, but in fact since then, again, Alison McDonald's study provides no support for it.

However, the argument in 146 remains sound, which is that because in a factory using amosite only, and not chrysotile at all - namely the New Jersey factory making insulation material, there were, I think, the last time, eleven mesotheliomas. This... and some of these were people who were exposed for relatively short periods, and also there have been cases of mesothelioma reported amongst their relatives, none of whom were exposed to chrysotile.

I am persuaded that most of the mesotheliomas occurring in American insulators are due to amosite, not chrysotile.

Furthermore, if one looks at the table eleven, which I referred to this morning, you will find that a very substantial proportion of them are peritoneal mesotheliomas, which have never been described in relation to chrysotile.

But again, I want to say that I do not believe that there is a difference in kind between chrysotile and amphiboles in respect of the propensity to produce mesothelioma. There is a difference in degree. But I think that up to the present time the difference in degree has been of such magnitude that it requires special control. It is at least an order of magnitude, and perhaps as much as thirtyfold, as Alison McDonald has said.

DR. DUPRE: To make sure then, that I understand paragraph 146 in the context that you have just advised me, I guess I should de-emphasize the word mixture that appears in there,

DR. DUPRE: (cont'd.) and emphasize the word amosite? It is the significant presence of amosite, rather than...

THE WITNESS: Yes, it's amosite. It's amosite.

You see, the point...the difficulty was that we were faced with, we were persuaded without any question that mesotheliomas in the United Kingdom are closely associated with crocidolite.

Now, in North America, we find these three populations of insulators, none of whom were exposed to any amount of crocidolite, who have substantial numbers of mesotheliomas.

Most of them were exposed to both amosite and chrysotile. Which was it? Was it one or the other, or was it an interaction of both?

Now, on the basis of the fact that the amosite factory, where there was no chrysotile, got mesotheliomas not only amongst the men, but their relations, it seems to me...and for the other reasons that I've mentioned, the fact that the amosite mine, there have been at least five, but five we know of, I think that amosite is probably responsible for most of the mesotheliomas amongst American insulators.

DR. DUPRE: Just one last question, Dr. Acheson, and this is really, I guess, in the way of asking for a reaction, on page nineteen, at paragraph seventy-seven, you go on to what then became in your papers, in any of a number of paragraphs, what I found an altogether instructive discussion of the extent to which what we are looking at is the capacity of asbestos to split into finer fibers, or on the other hand, their insolubility and resistance to destruction by acid and so on.

Among other places, there are a number of very instructive comments, but for example in paragraph eighty-five on page twenty, the last sentence, "Nevertheless as far as any rate of mesothelioma is concerned, the evidence"... this would be epidemiological evidence..."suggests that it may be the physical configuration of fibers rather

DR. DUPRE: (cont'd.) "than their chemical composition which dominate the issue of carcinogenicity".

5 My question is simply in terms of inviting your reaction to a proposition that has been put to us by another expert witness that we've had here, and as I understand it, this point would lend some considerable support to chemical composition in the sense that we were invited to consider very closely the possibility that the relatively-low acid resistance of chrysotile fibers may induce them to dissolve in the acidic content of human tissue much more readily than is the case with other fiber types.

10 Is this proposition that was put to us by another expert witness something to which, in your view, you might well give some weight or continue to pursue?

15 THE WITNESS: Well, I cannot testify on whether the acid available in the human body will or will not dissolve chrysotile. That must be a matter of fact which the biologists, cellular biologists can answer. I know that there has been a controversy, which I understand still rages, as to why there is much less of the inhaled chrysotile than the inhaled crocidolite and amosite present in the lungs of animals at the end of an experiment, and there are two views.

20 One is that it is broken down or dissolved in a biological sense, and the other is that it is ejected so quickly after inhalation that it doesn't get into the lungs.

25 When we were considering our evidence, this did not appear to have been resolved. The importance of the first view, the one that you expressed, is that it has been suggested that in the process of dissolution the chrysotile is broken down in a way which renders it a complete carcinogen.

30 However, my own view is that...and this is particularly in respect of mesothelioma...that in that case you

THE WITNESS: (cont'd.) would expect to find less chrysotile in people with mesothelioma, than controls. You find neither less nor more, you find the same. Whereas with the amphiboles, you find more. It seems to me that in logic the most likely explanation is that the amphiboles are responsible for the mesotheliomas.

If the dissolution of chrysotile was relevant, I believe there would be less in the cases in the controls, and this has never been found.

DR. DUPRE: Thank you very much.

Mr. Laskin, a last question?

MR. LASKIN: Just one or two.

MR. LASKIN: Q. When M. Bazin took you to page ninety-five of volume two, and that table, my eye caught the other side of the page and I appreciate you didn't deal with the question of compensation, but I noticed a statement in that paragraph sixty-seven, opposite, which suggests the circumstances in which the United Kingdom compensates for asbestos-related disease, and I note the absence of asbestos-related lung cancer.

THE WITNESS: A. Yes.

Q. Is it the situation that in the United Kingdom that there is no compensation in that circumstance?

A. That is correct. Recently, the appropriate government committee has written to experts inviting advice as to whether or not, or in what way they should compensate asbestos workers who develop lung cancer.

We compensate people with evidence of asbestosis, which is agreed by a pannel of experts called the Pneumoconiosis Board. We also compensate people with mesothelioma, who have been exposed to asbestos. At the moment we do not compensate people with lung cancer.

Q. In respect of mesothelioma, is there any requirement for any minimum past exposure history?

A. I can't answer that question, I'm afraid. I do not know.

5 Q. Just one final question which was prompted by a question that Linda Jolley asked you, when you looked at the alternatives to the optical microscope in the workplace, and looked at the electron microscope, did you look at both the scanning electron microscope and the transmission electron microscope?

10 A. I don't recollect that issue being debated. I recollect the decision, which I think was taken without much debate, that in practical terms the only way to do it in the immediate future is to go on in the way we had done it in the past, but that it was urgent for further studies to be made to improve the method, as a research enterprise.

15 MR. LASKIN: Thank you very much, Dr. Acheson.

DR. DUPRE: Dr. Acheson...

Do you wish to have another...

20 MR. McNAMEE: With your indulgence, Doctor, and also the indulgence of the Commission, the fact that you seem to be interested in this sort of international record keeping and we might not have anybody else to ask this question, the doctor tells me I'm being an idealist, but I'll ask it anyway: You have indicated that fifty percent of United Kingdom people apparently die in the hospital, and it's my feeling they are already subject at the time of being admitted and during the course of the treatment of their last illness, a lot of records are taken of medical and background information - most of it of a noncontroversial nature. I mean, they don't necessarily put down the fellow is a lush, or whatever he is, but this information is available and retrievable easily at the time of death, and I'm just wondering if you and your colleagues have given any thought to a more comprehensive international form of death certificate containing more than just a coding of death, containing certain noncontroversial background information that could be, say, put on

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5 Q. (cont'd.) a printed form at the back and that might save...you know, I mean, there might be two million death certificates in the United States...and a lot of this could be, in fifteen minutes, put in by the hospital and also by the doctor. It saves a lot of looking later on.

10 THE WITNESS: A. The question of death certification is currently under review in the United Kingdom. Basically the difficulty is, or one of the difficulties is that much of the data, apart from the medical certificate of death, comes from the relative, and there is a strict limit in the circumstances of a bereavement what sort of interview the registrar can reasonably undertake with a widow or widower, or other relative.

15 But there are certainly attempts being made to improve, to recommend improvements in the amount of data about occupation and industry, and to extend quite markedly the analysis of the effects of occupation and industry in women, which currently is not looked at at all. And also in the case of deaths of children and infants, to take account of the occupation of the parents.

20 But I think in practical terms there is a limit to what one can usefully record at death, and perhaps that's about the end of it.

MR. McNAMEE: Thank you, Dr. Acheson.

25 DR. DUPRE: Well, Dr. Acheson, you undertook a long journey to give us an exceedingly long day. Let me assure you we are most grateful indeed, sir.

THE WITNESS: Thank you, sir. Thank you for the courtesy of your questions, and for the opportunity to come and give evidence to your Commission.

DR. DUPRE: Thank you, indeed, sir.

30 Do I understand correctly, counsel, that tomorrow morning we do not convene until ten-thirty?

- 123 -

Acheson,

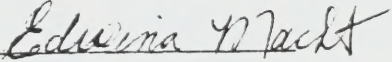
MR. LASKIN: That's correct, Mr. Chairman.

DR. DUPRE: We shall convene at ten-thirty tomorrow morning.

Thank you.

THE INQUIRY ADJOURNED

THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS


EDWINA MACHT

